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American Political Science Review

Table of Contents

SURPLUS/WITHDRAWN
W.K.U. LIBRARIES

ARTICLES

The Moral Sense
James Q. Wilson 1

A Model of the Political Economy of the United States
Alberto Alesina, John Londregan, and Howard Rosenthal 12

Government Formation and Endogenous Parties
David P. Baron 34

Reconstructive Democratic Theory
John S. Dryzek and Jeffrey Berejikian 48

General Deterrence between Enduring Rivals: Testing Three Competing Models
Paul Huth and Bruce Russett 61

Is Talk Really Cheap? Prompting Conversation between Critical Theory and Rational Choice
James Johnson 74

The Supreme Court as a Countermajoritarian Institution? The Impact of Public Opinion on Supreme Court Decisions
William Mishler and Reginald S. Sheehan 87

A Theory of Voting Equilibria
Roger B. Myerson and Robert J. Weber 102

Guns, Butter, and Anarchy
Robert Powell 115

Socrates' Aspasian Oration: The Play of Philosophy and Politics in Plato's *Menexenus*
Stephen G. Salkever 133

RESEARCH NOTES

Contextual Determinants of Feminist Attitudes: National and Subnational Influences in Western Europe
Lee Ann Banaszak and Eric Plutzer 147

What Moves Policy Sentiment?
Robert H. Durr 158

CONTROVERSY

Polarity and International Stability
Manus I. Midlarsky; Ted Hopf 173

NOTES FROM THE MANAGING EDITOR 181

BOOK REVIEWS

Review Essay: The Causes and Consequences of Divided Government: Toward a New Theory of American Politics?
David W. Brady 189

Political Theory 195

American Politics 206

Comparative Politics 230

International Relations 253

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ARTICLES

American Political Science Review Vol. 87, No. 1 March 1993

THE MORAL SENSE

Presidential Address, American Political Science Association, 1992

JAMES Q. WILSON

The central problem for social science is to explain social order. How do people manage to live together? One can discern two ways of answering that question. The first view is normative and communal: people learn from their culture customs that provide an internal compass guiding them to act in ways that minimize conflict and ensure comity. The second view is rationalistic and individualistic: order is created by explicit and implicit agreements entered into by self-seeking individuals to avert the worst consequences of their predatory instincts. In the first view, order is natural and prior to any social contract or government institution; in the second, it is contrived and dependent on agreements and sanctions. Rules are obeyed in the first case because they have moral force, in the second because they convey personal advantage. In the first view, compliance is automatic and general; in the second, it is strategic and uncertain.[1]

The normative view has been under heavy attack for several decades for at least three reasons: it seems to imply a complacent functionalism; it appears to minimize or deny the value of conflict; and it lacks the theoretical power found in the assumption that people always seek their own interests. I believe that one can grant, up to a point, all of these objections and still be left dissatisfied with the alternative, namely, that social order is contrived, based on calculation, and dependent on individual assent.

I wish to reestablish a version of the normative view. My argument is that while conflict within societies is ubiquitous and diversity among them obvious, people everywhere have a natural moral sense that is not entirely the product of utility or convention. By *moral sense* I mean a directly felt impression of some standards by which we ought to judge voluntary action. The standards are usually general and imprecise. Hence, when I say that people have a moral sense, I do not wish to be understood as saying that they have an intuitive knowledge of moral rules. Moral rules are often disputed and usually in conflict; but the process by which people resolve those disputes or settle those conflicts leads them back to sentiments that seem to them to have a worth that is intuitively obvious. These sentiments constitute the fundamental glue of society, a glue with adhesive power that is imperfect but sufficient to explain social order to some degree. The philosophers of the Scottish Enlightenment, in particular Francis Hutcheson writing in 1742, David Hume in 1740, and Adam Smith in 1759, explored with care and subtlety the reasons why certain sentiments commend themselves to us as worthy. I am under no illusion that I can improve on what they accomplished; but I hope to show, by drawing on the social and biological sciences, that their fundamental claims are consistent with much of what we have learned since the mid-eighteenth century.

One can infer the existence of a moral sense from behaviors that cannot easily be explained by even enlightened self-interest. There is less crime than one would expect from the probability of detection and punishment.[2] Even in the poorest neighborhoods, a complete breakdown of law and order does not lead most people to engage in looting. There are more obligations honored than one can explain knowing only that it is often useful to honor them. For example, we sometimes keep promises when it is not in our interest to do so, we often vote in elections even though we cannot affect the outcomes, we make charitable donations to organizations that confer no recognition on us, and some of us help people in distress even when no one is watching to applaud our good deed.

It will be objected that voting, donating, and helping are far from common actions and thus that little of moral significance can be inferred from their occasional performance. I do not fully accept that objection, but I recognize its force. Let me turn, then to behavior that is well nigh universal, that cannot be explained by individual calculations of utility or by negotiated social contracts, and that has obvious moral implications in any plausible meaning of the word *moral*: child care.

THE CHILD AS RECIPIENT AND SOURCE OF THE MORAL SENSE

People bring children into this world and nurture them through long years of dependency. They do this with no hope of immediate gain and every expectation of sleepless nights, financial burdens, and daily vexations. David Hume, in his attempt to base morality upon sentiments, was led ineluctably to the parent–child relationship as the founding sentiment. Justice, he argued, was an artifice, a set of rules useful because they make people secure in their property and enable them to transmit it in an orderly fashion. But why do people care about the transmission of property? Because of "the natural affection, which they bear their children" (1978, 486). That natural affection implies an obligation; people everywhere praise those who care for their children and despise those who do not (p. 478). Note how easily Hume inferred an "is" statement from an "ought" statement scarcely eight pages beyond the famous passage in which he suggested that this cannot be done.

Two objections may be made to this view. Scholars bent on finding self-serving explanations for behavior will argue that parents produce offspring because the latter are useful as unpaid laborers and future breadwinners. While this no doubt occurs and may explain why some parents feel a duty to their children, it cannot explain why children should feel any obligation to their parents. The youngsters are free-riders who benefit from nurturance whether they later support those who nurtured them or not. Yet children feel and act upon obligations to their parents despite the fact that such actions are unprofitable.

Scholars determined to explain all behavior as culturally determined and thus morally relativistic will argue that child care is by no means universal. Many writers have asserted that a sense of childhood is a recent invention, preceded, at least in Europe, by centuries of neglect, abuse, death, and abandonment (Aries 1962; de Mause 1974; Shorter 1975; Stone 1977). If mothers still kill their own babies and if parents have only recently stopped sending them to foundling homes, the love of children must be a recently acquired and thus socially learned disposition. In the words of Lloyd de Mause, the history of childhood is a "nightmare from which we have only recently begun to awaken" (1974, 1). The further back in time we go, the more likely the child was to be killed, abandoned, beaten, terrorized, and sexually abused. Edward Shorter gathered data on the use of wet nurses and foundling homes in the eighteenth century that, to him, bespoke a "traditional lack of maternal love" (1975, 203–4). A sense of childhood—and the love of children—is seen as a modern invention, ascribed variously to religion, capitalism, and the Enlightenment.

It is odd that this view should have had so large an impact, since we already knew from studies of contemporary primitive societies, such as the San of the Kalahari Desert, that children are not only loved but indulged without benefit of modern science, enlightened teaching, or capitalist requirements (Konner 1982, 301–4; see also LeVine 1970). It is even odder that conclusions about people's feelings about children were inferred from data about how children were treated, since the treatment of children might well have been shaped as much by circumstances as by attitudes (Anderson 1980; Macfarlane 1979). For example, wet-nursing was used by women who, because of their employment in agriculture, could not breast feed their own infants and for whom no other safe source of food was available (Sussman 1977; Wrightson 1982). In any event, wet-nursing was far less common than Shorter would have us believe. Perhaps 10% of all Parisian women, beyond that small percentage who were physiologically unable to nurse, put their children out to wet nurses (Ozment 1983, 220, n. 83; cf. also pp. 118–19). There is no evidence to support the assumption that wet nurses or foundling homes were used because most or even many parents were indifferent to their children.

Such direct evidence as we have about the feelings of European parents and children toward one another is inconsistent with the view that a caring family is a recent invention. The painstaking research of Linda Pollock (1983) in over four hundred diaries and autobiographies, including many written by children, suggests quite strongly that British and American parents and children from the sixteenth to the nineteenth centuries felt toward each other much what we feel today: mutual attachment and great affection.[3]

But surely the existence of infanticide throughout history confirms the purely conventional nature of family attachments. How could a natural sentiment—affection for a child—ever coexist with the deliberate killing of that child? It is a profoundly important question and one that cannot be entirely resolved on the basis of the available historical and anthropological evidence. In his brief but chilling history of infanticide, William L. Langer noted that it has existed everywhere since time immemorial as an accepted procedure for disposing of deformed infants and limiting the size of the population during periods of extreme privation. Jews had always condemned the practice (Noonan 1965, 86); but only with the advent of Christianity did there begin, in Europe at least, the widespread condemnation of the practice on moral grounds, and only with the spread of Christianity did the secular authorities make it a crime (Langer 1974; see also Herlihy 1985, 23–27 and Noonan 1965, 85–87). In hopes of providing an incentive that would reinforce the sanctions of the criminal law, many states created foundling homes in which mothers could leave their unwanted infants. These hopes were realized beyond the capacity of the system to accommodate them. In 1833, 164,319 babies were left in French foundling homes. At about the same time, one such home in Saint Petersburg had 25,000 infants on its rolls, with 5,000 being admitted yearly (Langer 1974, 358–359). The crowding was so great, and regulation so lax, that conveying a baby to a foundling home was often tantamount to sentencing it to death from neglect.

While this grisly history confirms how often infants were killed or abandoned, it is not very clear about the sentiments and motives of those who did it. Langer suggests that the motive for infanticide reflected extreme circumstances, typically a child who was either deformed or beyond the capacity of its poor parents to feed. To this must be added the threat of social stigma, moral obloquy, or penal sanction faced by unwed mothers. But these are only suggestions, not conclusions based on a close study of parental feelings. For the history of infanticide to shed light on the existence of a moral sense, it is essential to know how the parents, and especially the mothers, felt about what they did. What proportion disposed of the baby without remorse as a matter of convenience, and what proportion did so in anguish and out of necessity? Langer attributed the decline in infanticide in the late nineteenth and early twentieth centuries to the advent of modern contraception coupled with more stringent state regulation. This is a troubling hypothesis, for it implies that convenience dictated whether the baby would be killed or not. It neglects entirely what may have been the more important causes of the decline: a rise in the standard of living sufficient to enable poor parents to support several children, a change in the attitude toward unwed mothers great enough to make it thinkable to keep an illegitimate child, and an improvement in medical care adequate to ensure the ultimate good health of sickly infants.

The only way to assess the moral significance of infanticide is either to examine the feelings of the parents directly or to consider what happens when the conditions giving rise to it change. As with child neglect more generally, it is a mistake to infer sentiments from actions. In the modern world, infanticide still occurs; but there are no reliable data on how often (Dickemann 1984, 433; Scrimshaw 1984, 449–52). The closest thing we have to systematic data is a survey of 112 preindustrial societies from which the authors concluded that infanticide was "common" in about a third of them (Divale and Harris 1976; see also Scrimshaw 1984). The word *common* was not defined, nor does anyone have any idea how many times it actually occurs in any society.

So far as one can tell, infanticide occurs today under essentially the same conditions as in the past but less frequently: there is so little food that the child cannot be fed (especially a problem with twins), or the child is born so deformed or sickly that it chances of survival are slight (Daly and Wilson 1984, 488–95; idem 1988, chap. 3; Scrimshaw 1984, 444–60). Infanticide may also occur when the child's paternity (hence legitimacy) is in dispute. Of the 112 instances in which a cultural justification for infanticide could be found in the anthropological literature, all but 15 involved food shortages, deformity, or uncertain paternity (Daly and Wilson 1987, 207; see also Konner 1990, 173–76). Less common are instances of female infanticide in order to minimize the number of girls for whom dowries must be provided (Dickemann 1979, 456; Konner 1990, 173–76). No sentiment is sovereign; each must compete with others. A mother's affection for one infant must compete with her affection for another and with her own desire to survive. In a poor area of Brazil, mothers cope with this competition by not naming the baby until its survival seems assured (Scheper-Hughes 1987, 203–4).

That a mother's affection for her infant is not sovereign, however, does not mean that it is natural. Suppose for a moment that mother–child attachments were purely a matter of convention such that infanticide was governed by personal advantage or cultural practice. Under those circumstances, one would expect to find some societies—perhaps many—in which babies were killed even though food was plentiful, paternity certain, and the child healthy. After all, even healthy, easy-to-support children can be, for many years, a great nuisance. We would also expect to find some societies in which children were killed in the second or third year of life, rather than immediately after birth, especially since (as every parent knows) a two-year-old child is often a greater burden than a newborn infant.

But in fact, all of the predictions that follow from a purely relativist view of human nature are, so far as one can tell, false. When economic stresses end, infanticide becomes far less common and is almost always made a criminal act. Healthy babies of certain paternity are rarely destroyed. Infanticide almost never occurs after the first year of life; indeed, it rarely occurs except during the first few hours of life (Daly and Wilson 1987, 208–9; Idem 1988, 75–76; Eibl-Eibesfeldt 1989, 194; Scrimshaw 1984, 440, 448–49; Trevathan 1987, 231–2). That is because infanticide must be committed before bonding takes place. If the baby does not die almost immediately, the mother's distress is very great, at least in the few instances in which scholars have been on the scene to record the events (Eibl-Eibesfeldt 1989, 193–94)].

SOCIABILITY AND THE EMERGENCE OF THE MORAL SENSE

The view that there is not a natural moral sense sufficient to account, to any significant degree, for social order began, I believe, when philosophers argued that the human mind was a tabula rasa. If everything, including morality, had to be learned, then anything could be learned. Cultural relativism was the inevitable result of viewing human nature as wholly passive and completely malleable.

Modern science has destroyed that view. It is now clear that nature has prepared the child to be an active participant in his social development and disposed him to see and judge the world in moral terms. In the words of certain anthropologists, the child is an "intuitive moralist" (Shweder, Turiel, and Much 1981).

Newborn infants engage in social activity before they are taught it. They root, suck, and express distress at the sound of other babies crying. They prefer human sounds to other sounds, female sounds to male ones, and maternal sounds to other female sounds (Eibl-Eibesfeldt 1989, 200; Field 1990, 27–39; Hay and Rheingold 1983). This prosocial behavior is not learned. Infants born blind will smile though they have never seen a smile; infants born both deaf and blind will laugh during play, though they have never heard laughter, and frown when angry, though they have never seen a frown (Eibl-Eibesfeldt 1989, 30–31). The newborn infant can tell its mother's voice from that of another. It will imitate several facial and hand gestures within two weeks of birth and some gestures within 32 hours of birth (Meltzoff 1988; Meltzoff and Moore 1977). Most, if not all, of the universal human facial expressions—those expressing happiness, sadness, surprise, interest, disgust, anger, and fear—can be observed in the newborn child (Field 1990, 61).

Within two weeks infants will reach for a presented object (Eibl-Eibesfeldt 1989, 53) and will cry at the sound of another baby crying (Radke-Yarrow, Zahn-Waxler, and Chapman 1983, 479) but not at the recorded sound of their own crying (Field 1990, 31; Sagi and Hoffman 1976). Within six months babies can tell the difference between the face of a friendly and an unfriendly adult (Radke-Yarrow, Zahn-Waxler, and Chapman 1983, 480). Within two years, children will share toys, and offer help, and console others who are in distress (p. 481).[4]

The mother responds to these prosocial behaviors with nurturance, affection, and communication. She smiles at the child's smile and laughs at its laughter, picks it up when it cries, feeds it when it is hungry, and plays it when it is bored. Some people believe that if you reward behavior it will be repeated. One might infer from this general truth the particular claim that if you pick up a crying baby, it will always cry and that if you play with a fussy baby, it will always fuss. Not so. The natural sociability of the child inclines it to acquire greater autonomy and confidence, not greater dependence and manipulativeness, when its desire for attachment is met with an equivalent response from its parent.

This is the great paradox of attachment. Bonded children will grow up to be, not dependent, but independent, at least within such latitude as the culture allows. Human infants become attached to humans who make eye contact, whether or not they supply food. Bonding, once it has occurred, will so persist despite punishment that abused children will remain attached to abusive parents. It is clear from these facts that bonding is driven by powerful biological forces and is not simply the result of a utility-maximizing organism engaging in whatever behaviors bring it immediate rewards.

The child has within it, so to speak, a template that makes some kinds of learning quite easy and others very difficult. For example, a child can be conditioned to fear rats and spiders but not to fear opera glasses (Rachman 1990, 156–58). He is preprogrammed, if you will, to discriminate between things that are relevant to his life (because they can hurt) and things that are not,[5] much as he is preprogrammed to learn language at a certain time and in a certain way (Lenneberg 1972).

This natural sociability shapes the child's relationships with its parents, siblings, and peers. In all of these encounters, the child is not a passive organism repeating whatever acts are rewarded; nor is it a blank slate on which the world can write any message. The child is an active partner not only in shaping, but in judging, its experiences, as is evident when we consider the emergence of two of the moral sentiments, sympathy and fairness.

SYMPATHY

Children are by nature sociable; in the family they learn to extend sociability into generosity. This extension requires the instruction and example of parents, other kin, and older playmates; but the original impulse requires no instruction. The innate sociability of children makes them sensitive to the moods and actions of others. At first they try to control those moods and actions simply for their own pleasure; later they grasp that what pleases them may not please others, and so they act on the basis of some knowledge of the feelings of others. Children learn without much instruction that their own happiness is in some ways affected by the happiness of other; with some instruction, they learn that the happiness of others can be improved by modest sacrifices in their own well-being. Their own experiences and the teachings of others produce habits of action that routinely take into account the feelings of others. All this occurs early in life, before the children have understood sermons, mastered moral precepts, or read cautionary tales.

No infant needs to learn to assert its own needs; it cries when it is hungry or in distress. Until recently, however, many psychologists assumed that it had to learn everything else. Its capacity for sympathy was, in this view, an acquired characteristic. No doubt how a child is raised will affect the extent to which it is empathic or altruistic, but we now know that the infant brings to its own rearing a keen sensitivity to the distress of others. As early as 10 months of age, toddlers react visibly to signs of distress in others and often becoming agitated; when they are one-and-a-half years old, they seek to do something to alleviate the other's distress; and by the time they are two years old they verbally sympathize, offer toys, make suggestions, and look for help (Dunn 1987; Dunn and Munn 1986; Zahn-Waxler and Radke-Yarrow 1982). Though these youngsters are no doubt expressing some learned reactions to distress, they seem prepared to learn these things. It is obvious that infants are biologically inclined to seek help and attention; it may also be that they are biologically inclined to offer help and give attention (Rheingold and Hay 1978, 119).

This innate sensitivity to the feelings of others—a sensitivity that, to be sure, varies among individuals—is so powerful that it makes us grasp not only the feelings of friends and family members but also those of some strangers, many fictional characters, and even animals. We wince when the motion picture hero is threatened and exult when he is triumphant; we are disturbed by the sight of a wounded dog and pleased by the sight of someone else's baby.

It is sometimes argued that we display these feelings because it is expected of us or because we hope to curry favor with others or make ourselves seem worthy of reciprocal benevolence. That is certainly part of the story, but it is not the whole story. In a remarkable series of experiments, Bibb Latané and J. M. Darley (1970) showed that these explanations were probably wrong. They staged a number of "emergencies" in stores, offices, and laundromats, ranging from medical problems and fire alarms to thefts and disorderly conduct. In every case, a lone bystander was more likely to help the "victim" then was a group of bystanders.

This finding casts great doubt on the notion that altruism among strangers is merely a form of reciprocity by which helpers get credit for good deeds that can later be cashed in for other rewards, such as status. If altruism were really a self-interested investment in the future, then people should more frequently help victims when others can witness the good deed. But they do so less frequently. There is a social inhibition against helping that probably derives from the fact that in a group, the sense of personal responsibility is diffused. It is as if each person in a groups says to him- or herself, "Maybe somebody else will do it." When we are alone, we feel more keenly a sense of responsibility; we must answer not to the public but to the voice within.

Benevolence is often motivated by a desire for fame, status, or favors; but if that were all there was to it, our language would not be rich in words designed to distinguish selfish from selfless actions, kind from unkind persons, and heroism from bravado. If we really believed that altruism was merely reciprocity, we would purge our language of all such distinctions, and then the only difference between Tiny Tim and Scrooge would be age.

FAIRNESS

Perhaps the first moral judgment uttered by the child is, "That's not fair!" At first this claim may be largely self-interested, a way of making persuasive the real claim, "I want!" But at a very early age, the claim of fairness begins to take on the quality of a disinterested standard. It does so because fair play—taking turns, sharing toys, following rules—is a necessary condition for the child to satisfy its natural sociability. Judy Dunn (1988), who closely observed children between the ages of 18 and 36 months, found that about half spontaneously offered to share things and noted instances, familiar to every parent, of even younger babies offering a toy, pacifier, or piece of food to another person. These offerings reflect a desire on the part of the toddler to win approval, initiate play, or maintain contact. Around the world, children offer food as a way of establishing friendly relations even before they are able to talk (Eibl-Eibesfeldt 1989, 340–41). This tendency to share increases with age and is accompanied by a rapid growth in the sense of what rules ought to govern play and contact.

Jean Piaget formulated his theory of the development of moral judgment by watching children play marbles. What struck him most forcibly is that the complex and subtle rules of that game are not taught by adults to children, they are taught by children to each other (Piaget 1965, 14). Out of ordinary play and interaction there emerges a fairly clear sense of rules and justifications: "principles of possession, positive justice, [and] excuses on grounds of incapacity or lack of intention" (Damon 1988, 172). These rules are not specific to particular situations but are understood generally, so that they can be applied differently in different contexts without sacrificing the principle underlying the rule.

Children, from infancy on, court other people. They differ in the skill and enthusiasm with which they do this and the clarity and consistency of the rules that they infer from this courtship, but the process is not driven by self-interest narrowly conceived. A child, especially a two-year-old, is learning that it has a self that is different from the self of others; but it is also beginning to learn that its self requires the presence of others to achieve happiness. In the language of economists, children learn that utilities are interdependent—that one's happiness depends to some degree on the happiness of others—long before they can say "interdependent." Children learn that they ought to obey certain rules because it pleases others at the same time that they learn that breaking rules can be fun (up to a point) because it teases others.

By the time that they are in elementary school, the idea of fairness has acquired a fairly definite meaning: people should have equal shares. But once the equality principle is grasped, exceptions to it become apparent. For example, it does not seem right that a lazy boy should be paid as much as an energetic one when working on the same task. By the time they have left elementary school, children will go to great lengths to discuss and weigh competing principles (merit, age, need, etc.) for allocating things in a fair way. Most striking about this process, notes Ann Cale Kruger, is that they discuss these matters almost entirely without reference to adult authority figures or adult rules, regardless whether they are being interviewed by adults or secretly overheard (cited in Konner 1991, 299–301). Far from expressing an internalized set of adult rules or looking furtively over their shoulders for any sign of adult power, older children discuss, in sophisticated detail, principles of justice that have evolved out of their own interactions. Their affiliation with others in natural social groupings is the continuing source of their moral judgments.

A vast body of research on adult behavior provides compelling evidence for the importance of fairness as a guide to how we behave. In these studies (and here as well), *fairness* is defined much as Aristotle defined distributive justice: "What is just . . . is what is proportionate" (*Nichomachean Ethics* 1131b17)—that is, things should be divided among people in proportion to their worth or merit. In modern equity theory, a division of something between two people is fair if the ratio between the first person's worth (effort, skill, or deeds) and gains (earnings, benefits, or rewards) is the same as the ratio between the second person's worth and gains.

In a famous set of studies in the 1960s, various experimenters hired men to conduct interviews, paying them on a piece-rate basis. During the hiring, the "employer" (an experimenter) made clear to some men that he thought them unqualified (implying that they would be overpaid for the work to be done) while saying to others that they were fully qualified (implying that they would be equitably paid). The men were then sent out to work. Those who were made to feel unqualified (and hence overpaid) produced fewer but better interviews than did the men who were given to believe that they were being fairly paid. When some employees were made to feel that they were underpaid (i.e., that their skills were worth more than they would earn), they produced far more interviews (but of lower quality) than did employees who believed they were fairly paid (Adams 1963; Adams and Jacobson 1964; Adams and Rosenbaum 1962; Lawler and O'Gara 1967; Walster, Walster, and Berschied 1978).

This is not what one would expect if people were only interested in maximizing their income. Both the "overpaid" and the "equitably" paid workers earned the same amount per interview completed. If getting the most money was all that mattered, both groups would try to complete as many interviews as possible and the earnings of each group (the employees were randomly assigned) would be identical. What their employer thought of them would be irrelevant. The fact that the "overpaid" workers did less work (thereby sacrificing earnings) but did work of higher quality (thus sacrificing effort) can be explained in terms of their concern for equity.

There are many circumstances in which the self-interest of a person is not at all engaged, yet that person experiences—and often acts upon—strong feelings of fair play. Most of us are outraged at members of Congress who bounce checks even though the cost to us is close to zero. We are upset if, while waiting in a line to buy tickets to attend the theater, someone cuts in line ahead of us even though the addition of one person to the line almost certainly will not affect our chances of entering the theater and only trivially affect our choice of seats. But we believe that we have been treated unfairly because, by arriving earlier than the intruder, we have established a stronger claim (however tiny the difference) and thus are entitled to a greater reward (however small the increment). On the other hand, if the intruder can show that in fact he was there all night waiting for a seat, and only stepped away momentarily to get a cup of coffee, we will probably acknowledge his right to reclaim his place and will even (though less cheerfully) acknowledge the rights of his five companions, all being in the same position and all with the same prior claims.

EVOLUTION AND THE MORAL SENSES

The natural sociability of mankind gives rise to sentiments of sympathy, fairness, and reciprocity in every culture that we can imagine. Though custom will shape the reach of these sentiments by determining who is worthy of sympathy and what constitutes equality of worth or effort, the sentiments themselves emerge spontaneously. They do so because they are essential to human reproduction, family life, and small-group cohesion. These sentiments, in short, confer reproductive fitness. Ordinarily, parents who are innately disposed to care for their children produce more surviving children than do people lacking that disposition and so come to constitute an ever larger fraction of the gene pool.

Even John Stuart Mill readily admits this natural sociability but then, unaccountably (and quite unlike Hume and Smith), argues that "the moral feelings are not innate but acquired," implying that the "social feelings of mankind" and the "deeply rooted" sense that "there should be a natural harmony between his feelings and aims and those of his fellow creatures" have no moral content (1979, 30–31, 33). This was, as Charles Darwin was to remark eight years later, "extremely improbable": if the moral sentiments had no innate basis (i.e., had not been selected for by evolution), it is quite unlikely that so many of us would acquire them (1981; 71, n. 5; see also chap. 3).

The moral sentiments can lead to altruistic behavior, as when one individual risks his life that another might survive. Biologists have devised two theories to explain why altruism might spread in a population, rather than be extinguished by the greater rate of survival of wholly self-interested individuals. The first is the notion of inclusive fitness (Hamilton 1964), which holds that evolution will select for creatures that run risks for the benefit of others with whom they share genes and in proportion to the degree of that sharing. Flippantly but not inaccurately, J. B. S. Haldane (1955) put it this way: I will risk death to save my child from a raging river if the odds are at least two to one that I will succeed (because she shares roughly half my genes); but I will jump in the river to save my cousin only if the odds favoring success are seven in eight (because she has only one-eighth of my genes). Trying to save my grandmother makes no sense at all because, being past child-bearing age, she can pass on none of my genes to the next generation.

This calculus may explain why, on the average, we strive harder to save our own children than somebody else's, and it is certainly consistent with the fact that children are more likely to be abused by their step-parents than by their natural parents (Wilson and Daly 1987); but it cannot explain why we should ever run any risk at all of saving our grandmother, our adopted child, or our dog. Yet many people will jump into the river for grandmothers, adopted children, and even dogs.

Studies of adoptive families provide no evidence that parents of adopted children are any less loving, solicitous, or protective than are the parents of biological children. Mothers who have both an adopted and a biological child report no difference in their feelings toward them (Smith and Sherwen 1983, 95). If anything, adoptive parents are more protective and less controlling than biological ones (Hoopes 1982, 97–98), a puzzling finding if one believes that investment in child care is driven by a desire to reproduce one's genes. Adopted children report that they were loved as if they were natural children (Triseliotis and Hill 1990).[7]

People in primitive, as well as advanced, societies form strong attachments to animals (Serpell 1986, chap. 4). There are, of course, great variations in which animals are cherished; but beneath these variations there is a deeper constancy: in virtually every society and in virtually every historical period, people have been attracted to certain kinds of animals in ways that are hard to distinguish from the way in which we treat infants—difficult to explain in terms of economic necessity (the desire for food or help) and impossible to explain in terms of reproductive fitness.[8]

One can attempt to solve these puzzles of affectional behavior directed toward non-kin and nonhumans while remaining within a narrow interpretation of the evolutionary perspective by advancing the notion of reciprocal altruism: we engage in altruistic acts, such as helping nonrelatives, caring for adopted children, or being affectionate toward pets, in order to impress others with our dependability and hence to increase our opportunities to have profitable exchanges with these others (Alexander 1987; Trivers 1971). There is a great deal of truth in this; having a reputation for doing one's duty, living up to promises, and helping others will enhance one's own opportunities. Moral behavior is far more likely when utility conspires with duty, and the strongest moral codes are invariably those that are supported by considerations of both advantage and obligation (see Sidgwick 1981, 386–87).

But sometimes, sentiment alone, unsupported by utility, motivates our actions, as when someone makes an anonymous benefaction or a lone bystander helps an endangered person. While anonymous giving may be relatively rare, it is generally the case that a lone bystander is more likely to go to the aid of a threatened person than a bystander who is part of a group—the opposite of what one would predict if reputation enhancement were the motive for altruistic actions (Latané and Darley 1970).

Evolutionary biology provides a powerful insight into human behavior at the level of the species, but it fares less well at the level of daily conduct. This deficiency arises in part because evolutionary biologists ordinarily do not specify the psychological mechanism by which a trait that has been selected for governs behavior in particular cases (Cosmides and Tooby 1987). The strict and exclusive altruism of social insects is different from the more inclusive altruism of humans.

Sympathy for persons who are not offspring and creatures that are not human is a characteristic of almost all humans. Indeed, we regard as inhuman anyone who acts as if they had no feeling for others and we criticize as insincere people who merely feign such fellow feeling. If sympathy is widespread, it must have been adaptive; but what was selected for was not a simple desire for reproductive success: it was a generalized trait that both encouraged reproductive fitness and stimulated sympathetic behavior.

That trait, I suggest, is affiliative behavior. Evolution has selected for the attachment response: if infants and parents were not predisposed to develop strong attachments for one another, it would be impossible to provide for the postpartum development of the human central nervous system. A predisposition to attachment is necessary if a child is to be sustained for that long period of dependency and nurturance during which the human brain becomes fully developed.

But it is a mistake to suppose that the psychological predisposition for which evolution has selected will be as precise in its effect on human behavior as the social instinct is among ants. Our large brain almost guarantees that the effect of attachment will be complex and diffuse if for no other reason than that the human brain not only makes possible complex actions but also makes possible our imagining such actions. The predisposition to attachment is a pervasive but somewhat general drive that imperfectly discriminates between parents and parent substitutes; is evoked by adoptive, as well as natural, infants; extends to creatures that have just a few of the characteristics of the human infant; and embraces not only family but also kin and many non-kin.

One of the cues (in evolutionary jargon, "releasers") that stimulates this affectional response in adults is "cuteness"—by which I mean that set of traits by which we judge an organism to be delightfully attractive. We respond to certain features of people and animals in ways that suggest that we share a roughly common definition of cuteness: eyes large relative to the skull, chubby cheeks and a rounded chin, awkward movements, a cuddly epidermis, small size, and a distinctive smell (Eibl-Eibesfeldt 1989; Lorenz 1943; McKenna 1987, 161; Sternglanz, Gray, and Murakami 1977; Super 1981). Nonparents, as well as parents, respond to these cues; and the response extends beyond the human infant to other creatures with these infantile traits. I suggest that social scientists and moral philosophers have paid too little attention to the concept of "cute." People use the word all the time; philosophers and scientists almost never use it. Its frequent use suggests that it may refer to an important mechanism by which our moral sentiments are extended beyond ourselves and our immediate families.

There are other concepts, just as important and just as poorly understood, such as being a "fan," a "loyalist," or a "sentimentalist." Each suggests that our affiliative impulse is so strong that it can be evoked almost by remote control. We identify with people whom we do not know and who do not know us and with people who are entirely fictional. When we are in the audience of a play or motion picture, we are moved by the plight of imaginary people; when we watch an athletic spectacle or a military parade, we are moved by the exploits—and sometimes the mere sight—of people who are unaware of our existence. There is little in behavioral psychology or evolutionary biology that explains these emotions and their tendency to evoke in us moral sentiments.

THE MORAL SENSE, SOCIAL ORDER, AND MORAL CHOICE

Our moral sense, however weak or imperfect, helps explain social order because that sense grows out of, and reflects, the fact that we are social beings, dependent upon one another and because we are able to avail ourselves of the essential help of others, at least in the intimate precincts of life, only on the basis of understandings that arise spontaneously out of, and necessarily govern, human relationships: the need to show some concern for the well-being of others, treat others with minimal fairness, and honor obligations. This natural sociability and the patterns of sympathy and reciprocity on which it rests are the basis, in my view, of Aristotle's argument for natural law: man is by nature social, and social groupings aim at some good.

It can be put in the form of a thought experiment. Imagine people stripped of every shred of their social experiences and set loose in some Arcadian paradise, free to invent "culture." What would emerge? If they are young boys, the answer may be something akin to William Golding's *Lord of the Flies* (1962), though my guess is that a close study of abandoned children in war-torn nations would disprove even this hypothesis. But if they are men and women, what emerges would almost surely be something with strange customs, odd dress, and unfamiliar gods but invariably with familiar systems of infant care, familial obligation, kinship distinctions, and tribal loyalties.

The results of such a thought experiment taken together with the findings of modern science cast doubt, in my mind, on the philosophical value of imagining a man who is presocial, driven by a single motive, or unware of the main and necessary features of social life. John Rawls (1971) may ask us to imagine ourselves in an "original position" behind a "veil of ignorance"; but no human being is ever in such a position and, to the extent he is human, cannot possibly be ignorant. Locke (1690/1979) may ask us to believe that experience is the sole source of ideas; but if we accept that, we will have difficulty explaining why all children learn a language at roughly the same time and without having the rules of that language explained to them. Hobbes (1957) may ask us to believe that man is driven by the fear of violent death; but were that our overriding concern, we would not give birth to children or lavish so much care on them. Why expend so much effort on something so perishable, whose birth threatens the mother's life, and whose protection increases our vulnerability to the predation of others? Rousseau may imagine an equally implausible alternative, man born with no inclination to civil society; but no such man can exist and, if he did exist, could not learn goodness by reading *Robinson Crusoe* (1979, 184; see also Bloom 1978).

A proper understanding of our natural disposition to sociability not only helps explain social order, it provides the grounding for our judgments about that order. We cannot imagine praising a man who laughs while torturing an innocent baby (Thomson 1989); we cannot defend a principle that says that every man is entitled to be the judge in his own case. We are not limited to condemning Auschwitz contingently and ironically; we can condemn it absolutely and confidently (see Rorty 1989, 189).

Moral philosophy, like social science, must begin with a statement about human nature. We may disagree about what is natural; but we cannot escape the fact that we have a nature, that is, a set of traits and predispositions that limits what we may do and suggests guides to what we must do. That nature is mixed: we fear violent death but sometimes deliberately risk it; we want to improve our own happiness but sometimes work for the happiness of others; we value our individuality but are tormented by the prospect of being alone. It is a nature that cannot be described by any single disposition, be it maximizing our utility or enhancing our reproductive fitness. Efforts to found a moral philosophy on some single trait (the desire for happiness or the fear of punishment) or political philosophy on some single good (avoiding death or securing property) will inevitably produce judgments about what is right that at some critical juncture are at odds with the sober second thoughts of people who deliberate about what constitutes praiseworthy conduct and who decide, out of that deliberation, to honor the hero who risked violent death or to sympathize with the mother who sacrificed one child to save another.

As Ian Shapiro (1990, 296) has noted, much contemporary political theory is "locked into a series of antinaturalist assumptions about human nature"; for example, original positions, veils of ignorance, the priority of rights. Any reasonable theory must have a "view of human nature and human interests and an argument about the injunction for action this entails given a plausibly defended account of the pertinent causal structure of the social world" (ibid.).

Aristotle gave such an account, but his views became unfashionable among those who sought to base moral or political philosophy on a single principle (e.g., utility, liberty, or self-preservation), who worried about Aristotle's teleology, or who believed in the priority of the right over the good. But if one acknowledges that there is no single moral principle but several partially consistent ones and that neither happiness nor virtue can be prescribed by rule, one is better prepared for a more complete understanding of man's moral capacities, an understanding stated by Aristotle in phrases that in most respects precisely anticipate the findings of modern science. Though Aristotle's account is often dismissed as teleological (much as those of later scientists were dismissed as functionalist), his view does not involve any "mysterious non-empirical entities" (Nussbaum 1978, 60) or any suspiciously conservative functionalism.

There is certainly nothing mysterious or nonempirical about Aristotle's assertion that men and women unite out of a "natural striving to leave behind another that is like oneself" (*Politics* 1252a30) because a "parent would seem to have a natural friendship for a child, and a child for a parent" (*Nichomachean Ethics* 1155a17) or that "the household is the partnership constituted by nature for [the needs of] daily life" (*Politics* 1252b11). These are as close to self-evident propositions as one could utter. Only slightly less obvious, but still scarcely mysterious, are the arguments that "in the household first we have the sources and springs of friendship, of political organization, and of justice" (*Eudemian Ethics* 1242b1) and that "there is in everyone by nature an impulse toward this sort of partnership [that is, toward the city]" (*Politics* 1253a29).

These natural moral sentiments are an incomplete and partial guide to action. They are incomplete in that they cannot resolve a choice we must make between two loved persons or between the desire to favor a loved one and the obligation to honor a commitment. They are partial in that these sentiments extend chiefly to family and kin, leaving nonkin at risk for being thought nonhuman. Resolving conflicts and extending our sentiments across the high but necessary walls of tribe, village, and racial grouping—an extension made more desirable by the interdependence of cosmopolitan living—requires moral reasoning to take up the incomplete task of moral development.

These deficiencies can lead the unwary philosopher to suppose that if a sentiment does not settle everything, it cannot settle anything or to infer that if people make different choices, they must do so on the basis of different sentiments. The first error leads to logical positivism; the second, to cultural relativism; and the two together, to modern nihilism or, at best, to "liberal irony." A proper understanding of human nature can rarely provide us with rules for action, but it can supply what Aristotle intended: a grasp of what is good in human life and a rough ranking of those goods (Arnhart 1990; Salkever 1990).

Antinaturalist assumptions have impeded the search for explanations for social order, as well as efforts to justify different systems of order. Normative theories have stressed that order is the product of cultural learning without pausing to ask what it is we are naturally disposed to learn. Utilitarian theories have confidently responded by saying that we are disposed to learn whatever advances our interests without pausing to ask what constitutes our interests. And despite their differences in approach, they have both supported an environmental determinism and cultural relativism that has certain dangers. If man is infinitely malleable, he is as much at risk from the various despotisms of this world as he would be if man were entirely shaped by some biochemical process. Anthropologist Robin Fox has put the matter well: "If, indeed, everything is learned, then surely men can be taught to live in any kind of society. Man is at the mercy of all the tyrants who think they know what is best for him. And how can he plead that they are being inhuman if he doesn't know what being human is in the first place?" (1973, 13). Despots are quite prepared to use whatever technology will enable them to dominate mankind; if science tells them that biology is nothing and environment everything, then they will put aside their eugenic surgery and selective breeding programs and take up instead the weapons of propaganda, mass advertising, and educational indoctrination. The Nazis left nothing to chance; they used all methods.

Recent Russian history should have put to rest the view that everything is learned and man is infinitely malleable. After 75 years of cruel tyranny during which every effort was made to destroy civil society to create the New Soviet Man, we learn that people kept civil society alive, if not well. The elemental building blocks of that society were not isolated individuals easily trained to embrace any doctrine or adopt any habits; they were families, friends, and intimate groupings in which sentiments of sympathy, reciprocity, and fairness survived and struggled to shape behavior.

Mankind's moral sense is not a strong beacon light, radiating outward to illuminate in sharp outline all that it touches. It is, rather, a small candle flame, casting vague and multiple shadows, flickering and sputtering in the strong winds of power and passion, greed and ideology. But brought close to the heart and cupped in one's hands, it dispels the darkness and warms the soul.

Notes

An expanded version of this essay will appear in 1993 under the title *The Moral Sense* from the Free Press of New York City.

1. Emile Durkheim (1951, 1960, 1961) is an important exponent of the first view, Thomas Hobbes (1957) of the latter. For an analysis, see Elster (1989).

2. In 1986, three in one hundred thefts reported to the police resulted in prison sentences. Since at the most only one-fourth of all thefts are reported to the police, the true probability of imprisonment, given a theft is much less than one chance in a hundred (Langan 1991).

3. Pollock's conclusion is worth quoting, resting, as it does, on the most systematic review of the evidence that we have: "Despite the individual differences in child-rearing techniques, there are limits of variation. These limits are the dependency of the child and the acceptance of responsibility for the protection and socialisation of that child by the parents. From the material gathered here, it is clear that the vast majority of parents from earlier centuries were operating within these constraints" (1983, 271). A similar judgment about child care was reached by David Herlihy (1985) for medieval Europe, Steven Ozment (1983) for sixteenth-century Europe, and Keith Wrightson (1982) and Michael MacDonald (1982) for seventeenth-century England.

4. Mothers also behave in distinctive ways toward children whatever their cultural background. For example, whether they are right-handed or left-handed, the vast majority of mothers carry their infants on the left side; it is almost inconceivable that this is a learned behavior (Trevathan 1987).

5. There is a good deal of research on the limits of conditioning even among animals. Rats can be trained to avoid a

taste that is followed by induced nausea or to avoid a light that is followed by an electrical shock; but they cannot be conditioned to avoid a certain taste that is followed by a shock or avoid a light that is followed by nausea. They are preprogrammed to associate nausea with eating and light with shocks but not the reverse (Garcia and Koelling 1966; Seligman and Hager 1972).

6. Moreover, the much greater abusiveness of step-parents cannot be explained by economic differences, since natural parent and step-parent families do not, on average, differ in income (Wilson and Daly 1987, 225).

7. The fact that adopted children are at greater risk than natural children for psychological problems and conduct disorders does not invalidate the argument that the former are cherished equally with the latter. Adopted children have more personality problems because their *biological* parents had these problems, which have a large genetic component (Bohmann and Sigvardsson 1990; Cadoret 1990).

8. The animals that are most likely to become pets are those that have some of the characteristics of the human infant, such as eyes that are large relative to the face, a soft epidermis, and a prosocial disposition. The most common pets supply an object of affection that returns the affection through obedience, loyalty, purring, or posing (Serpell 1986, 66–68, 114–15).

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James Q. Wilson is Professor of Management and Public Policy, University of California, Los Angeles, Los Angeles, CA 90024.

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A MODEL OF THE POLITICAL ECONOMY OF THE UNITED STATES

ALBERTO ALESINA *Harvard University*
JOHN LONDREGAN *Princeton University*
HOWARD ROSENTHAL *Carnegie Mellon University*

We develop and test a model of joint determination of economic growth and national election results in the United States. The formal model, which combines developments in the rational choice analysis of the behavior of economic agents and voters, leads to a system of equations in which the dependent variables are the growth rate and the vote shares in presidential and congressional elections. Our estimates support the theoretical claims that growth responds to unanticipated policy shifts and that voters use both on-year and midterm elections to balance the two parties. On the other hand, we find no support for "rational" retrospective voting. We do reconfirm, in a fully simultaneous framework, the "naive" retrospective voting literature's finding that the economy has a strong effect on presidential voting. We find congressional elections unaffected by the economy, except as transmitted by presidential coattails.

The literature on macroeconomic fluctuations and general elections divides into three branches. One studies the impact of economic conditions on voting: economic fluctuations are viewed as predetermined, while electoral results are the endogenous variables (see, e.g., Chappell and Suzuki 1990; Erikson 1989, 1990; Fair 1978, 1982, 1988; Fiorina 1981; Kiewiet 1983; Kramer 1971). A second emphasizes that political parties pursue different policies that result in "partisan effects" on the economy.[1] A third line of research emphasizes the "opportunistic" behavior of politicians who try to manipulate the economy in order to increase their chances of remaining in office (see Alesina, Cohen, and Roubini 1992; Haynes and Stone 1987; Nordhaus 1975, 1989; Tufte 1978).

For the United States in the twentieth century, this literature discloses several regularities:

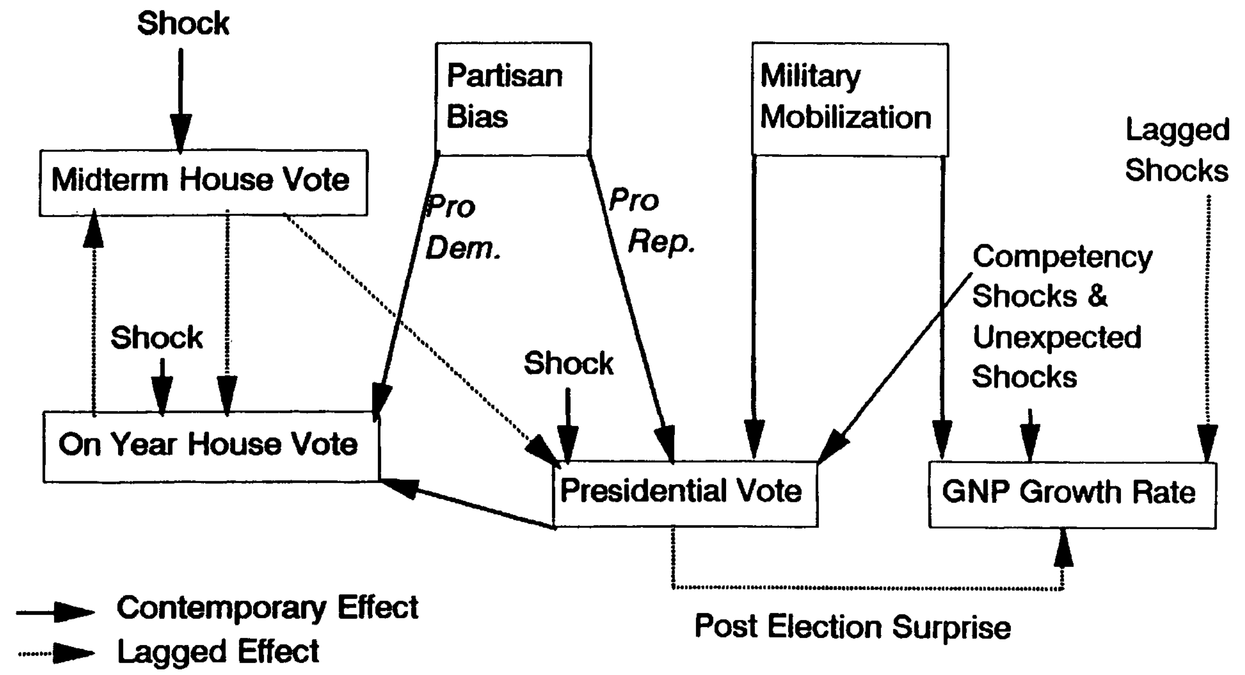
1. Presidential elections are strongly influenced by the business cycle. The vote share of the incumbent president's party's presidential candidate increases with the rate of gross national product (GNP) growth in the election year; other economic variables (e.g., unemployment, inflation) are less significant in explaining presidential results.[2]
2. Congressional vote shares are less sensitive to economic conditions (Alesina and Rosenthal 1989; Chappell and Suzuki 1990; Erikson 1990; Lepper 1974).[3]
3. There is a midterm electoral cycle where the party holding the White House loses plurality in midterm congressional elections (Alesina and Rosenthal 1989; Erikson 1988).
4. Since World War II, in the first half of Republican administrations, economic growth tends to decelerate, reaching its minimum during the second year of each term, while the economy grows more rapidly than average during the first half of Democratic administrations. In the last two years of each term, there are no significant differences between growth rates for Democratic and Republican administrations (Alesina 1988; Alesina and Sachs 1988).[4]
5. The rate of economic growth is not systematically higher than average in election years (see Alesina 1988; Alesina and Roubini 1992; Beck 1992; Golden and Poterba 1980; Hibbs 1987; McCallum 1978).

We construct and test a model consistent with the aforementioned regularities. In this model economic and electoral outcomes are jointly endogenous. The model, which posits rational choice by both voters and economic agents, is based upon four key ideas:

1. The two political parties are "partisan" and polarized. The Republicans, *relative* to the Democrats, are more concerned with containing inflation than with stimulating growth (Hibbs 1977, 1987). The Democrats also favor expansionary monetary and fiscal policies in order to support relatively large government programs.
2. Economic agents form "rational expectations," but wage "stickiness" prevents an immediate adjustment of wages to economic shocks or "news." Because the two parties follow different policies, uncertainty about the outcome of elections engenders uncertainty about postelection policies. Agents are forced to hedge this uncertainty in nominal wage contracts concluded prior to elections remaining in effect after the elections.[5] The actual postelection policies then produce real effects on economic growth. In particular, growth will be relatively high following the election of a Democrat, whereas recessions are most likely after a Republican victory (Alesina 1987).
3. Policy outcomes depend both upon which party holds the presidency and the relative share of seats in Congress. For instance, Democratic control of Congress "pulls" policy outcomes with a Republican president to the left. Voters take advantage of this institutional structure of "checks

FIGURE 1

Schematic Overview of the Model



and balances" to bring about middle-of-the-road, moderate policies (Alesina and Rosenthal 1989, 1991; Fiorina 1988, 1992). The midterm cycle is an instance of this type of moderating behavior.

4. Administrations vary in their degree of "administrative competence." For a given rate of inflation (expected or unexpected), a more competent administration is likely to produce more growth than a less competent one (Persson and Tabellini 1990). Since voters prefer more competence to less, elections will turn on not only partisan preferences but also efficiency arguments. However, a voter cannot observe "competence" directly but only its effect on the economy. Since the economy is also affected by technological innovations, oil price changes, wars, and other matters that have little to do with administrative competence, voters cannot immediately distinguish competence from "luck." A "rational" retrospective voter can only use available information to make a forecast of the incumbent's postelectoral competence. This forecast, as we shall show, leads economic growth to affect electoral results in a manner distinct from "naive" retrospective voting, where no attempt is made to distinguish good luck from good government.[6]

We shall integrate these ideas in a single theoretical model that encompasses both political and economic outcomes. The theory is followed by econometric estimation. Our theoretical model fares well in these tests, which support the theory on several key points including (1) institutional "balancing," manifest in the midterm cycle (the party controlling the White House loses vote share in the midterm elections); (2) the economy has a much greater impact on presidential elections than on congressional elections; and (3) the "partisan" effects on growth appear in the wake of presidential elections. On the other hand, we do not find support for "rational" retrospective voting, a result we shall later discuss at length.

Before embarking on a formal presentation of the model, we use the schematic shown in Figure 1 to provide the reader with a brief overview. The theoretical model leads to four equations in the empirical estimation: the GNP growth rate, the presidential vote, the on-year House vote, and the midterm House vote. Each of these equations has, as is common practice, independent random shocks. The GNP equation reflects the two aforementioned channels of government influence on economic growth. The first is "partisan" politics coupled with electoral uncertainty and nominal wage contracting. The second arises from variations in the competence of different administrations.

The partisan effect, present only at the beginning of each four-year term, arises because agents have to take into account that there are two possible inflation policies that will be pursued *after* the elections: high inflation if a Democrat wins the presidency or low inflation if a Republican wins. Wage contracts signed *before* the elections will be based on the *expected* postelection rate, which, since there is electoral uncertainty, lies in between the high Democratic rate and the low Republican rate. After the election, there is "unexpected" inflation represented by the difference between this *expected* rate and the *actual* rate implemented by the winner. If the Democrats win, for example, this unexpected "surprise" generates an upsurge in growth. The upsurge lasts, however, only for a couple of years, until new wage contracts have incorporated the higher, now expected, inflation policy. Subsequently, the economy returns to its "normal" rate of growth, implying that there is not a "partisan" influence on the rate of growth in the second half of each term.

The partisan growth model thus implies that the effects of macroeconomic policy on growth are short-lived. In contrast, Hibbs's (1977, 1987) model has persistent partisan effects on the economy. Moreover, in our model, an administration cannot use unexpected inflation to generate an upsurge in growth before elections. In fact, "rational" economic agents would predict such maneuvers and, in so doing, render them ineffective. Therefore, we do not predict a systematic preelectoral burst of growth, as in the Nordhaus (1975) "political business cycle."

In contrast to the partisan effect, the second, competence effect is present in election years and is the key to "rational" retrospective voting. Although we cannot measure "competence" directly, we can test a direct implication of the model. The model predicts that the intertemporal covariation in growth should be *higher* when there is no change in the party holding the White House than when there is one. This is because a change in control leads to a completely new level of competence while with no change the previous level persists. Therefore, the growth equation will be used to provide a very direct test of whether there is a basis for "rational retrospective voting." Indeed, preelectoral economic performance should matter to voters only if there is evidence that the government, as well as outside forces, has an important influence on performance. We will conclude against "rational" retrospective voting: the American electorate pays "too much attention" to the rate of GNP growth in election years.

We estimate both the partisan surprise and competence effects by using an autoregressive moving average representation of GNP growth similar to the unemployment equation in the seminal work of Hibbs (1977). Since we use only a first-order moving average, our representation is simpler than his. On the one hand, this simple representation renders the theoretical model tractable; on the other, statistical tests reject the need for more complexity in the empirical model. The estimation also includes a military mobilization variable as an exogenous measure for the effect of wars.

The presidential vote reflects both the electorate's partisan preferences *and* their evaluation of the incumbent's competence in managing the economy. In a standard "naive" retrospective voting model (e.g., Fair 1988), one would simply enter election-year economic performance as a predictor. But the rational "competency" model says that the voters should focus only on that portion of growth likely to persist after the election. This portion is represented by the shocks directed at growth in Figure 1. In addition to the test for growth, we also test, in the presidential equation, for a "rally 'round the flag" effect from military mobilizations.

The presidential equation and the on-year House equation jointly test the basic idea of institutional balancing. Balancing implies that if one party is advantaged in one institution, its opponent should be stronger in the other. We allow for partisan bias in both on-year voting equations. The on-year House vote further reflects our theoretical framework in allowing for "coattails" from the presidential vote. In contrast, the off-year House elections reflect only the balancing of the midterm cycle, which, as we shall show, is embedded in the lagged effect of the on-year House vote.

Recognizing the overbearing importance of incumbency in American politics, we include the lagged House vote in every voting equation. Indeed, these lagged variables greatly improve the fit. The incumbency advantage is not handled within our formal model for reasons of tractability. To some extent, the lags may proxy for serial correlation in the preferences of the electorate (such as an alleged liberal mood in the 1960s). But the lags have an additional, important role. Being strong in Congress currently facilitates retaining future control of Congress and, to a lesser extent, the presidency. This advantage is offset by the midterm effect, where winning the presidency today causes losses in Congress two years later. In fact, if all exogenous shocks were absent, our estimates show that the lags and the midterm effect would combine to produce a governmental cycle with divided government prevailing over most of the cycle.

THE THEORETICAL MODEL

We simplify the discussion here by treating a "period" as two calendar years. We use t to index periods.

The Economy

We consider a model in which nominal wage contracts are signed at the end of period t and cannot be revised until the end of period t + 1 (Fischer 1977). Thus, the rate of nominal wage growth equals ex ante expected inflation, since we assume that expected productivity growth is zero. Disregarding capital, a supply function for this economy can be written as follows:

$$g_t = \bar{g} + \gamma(\pi_t - \pi_t^e) + \varepsilon_t, \tag{1}$$

where g_t is the output growth, $\bar{g}$ is the "natural" rate of growth, π_t is the inflation rate, and $\pi_t^e = E(\pi_t|I_{t-1})$ is the rational expectation of inflation based upon the information available in period $t - 1$.[7]

The error ε_t consists of two components, which cannot be separately observed by the voters (nor by econometricians):

$$\varepsilon_t = \zeta_t + \eta_t. \tag{2}$$

The transitory shock ζ_t (independently and identically distributed with mean 0 and variance σ_ζ^2) represents unanticipated economic events beyond the scope of government control (e.g., oil price shocks, some technological innovations). The term η_t captures administrative "competence." In fact, *given inflation,* growth is higher, the higher η_t. In this context, competence can be interpreted as the administration's ability to avoid inefficiency and, generally speaking, to create an environment conducive to growth without inflation.

Competence does not disappear overnight. *Within parties,* the competency level exhibits inertia, evolving according to a first-order moving average, or MA(1), process:

$$\eta_t = \mu_t^R + \rho\mu_{t-1}^R \quad \text{if } R \text{ president at } t$$

$$\eta_t = \mu_t^D + \rho\mu_{t-1}^D \quad \text{if } D \text{ president at } t. \tag{3}$$

The disturbances are independently and identically distributed and satisfy

$$E(\mu_t^R) = E(\mu_t^D) = 0$$

$$\text{Var}\,(\mu_t^R) = \text{Var}\,(\mu_t^D) = \sigma_\mu^2.$$

This specification implies that we regard parties as ongoing organizations with competence that persists to the same degree whether the incumbent is reelected or a new president from the same party is chosen. The validity of the assumption can be argued by noting the relatively slow turnover of the cadre that forms cabinets. Note also that competence is given, not chosen by an administration. In fact, our model does not even require that the government knows its own competency level!

While for tractability, we do not consider congressional competence, it is reasonable to assume that variations of competence are more important to the presidency, since individual variations in competence are likely to cancel out in a legislative setting with large numbers of decision makers.

In our model, competence shocks persist for one period only, which (as we shall show), suffices to induce rational retrospective voting. Higher-order moving average (MA) processes could be introduced without changing our qualitative results as long as the shocks that precede a presidential election do not influence the competency level of the incumbent's party beyond a single presidential term (see Appendix).

The Two Political Parties

Two parties, D and R, compete for office, with no entry of third parties. All presidential and legislative candidates of a party have identical preferences. These preferences and those of voters are represented by utility functions that are, for tractability, quadratic in the inflation rate and linear[8] in output growth:

$$W^i = \sum_{t=0}^{\infty} \beta^t \left[-\frac{1}{2}(\pi_t - \bar{\pi}^i)^2 + b^i g_t \right] \quad 0 < \beta < 1, \tag{4}$$

where the index i takes on the values D, R for the political parties and generic value i for an individual voter. The parties and voters have a common discount factor β. The following inequalities capture the "partisan" nature of our model:

$$\bar{\pi}^D > \bar{\pi}^R \geq 0; \quad b^D > b^R > 0.$$

While the parties agree that output growth is desirable, they differ both in terms of their most preferred inflation rates ($\bar{\pi}$) and in the trade-off between output and deviations of inflation from its most preferred level (b). Party D is relatively more concerned with growth than with inflation. That the parties prefer positive rates of inflation to zero inflation is motivated by three features of the economy. First, inflation is a tax on nominally denominated assets. To finance public spending, it is optimal to distribute the burden of taxation as widely as possible, including the implicit tax represented by inflation. Second, empirical evidence suggests a negative correlation between *real* interest rates and the inflation rate. Third, Tobin (1972) and many others have argued that moderate inflation facilitates smooth adjustments of prices and wages, particularly (as appears to be the case) if the latter are rigid downward. Thus, the preferences on inflation can be interpreted as a reduced form of underlying preferences on public spending and real interest rates. We assume that the policymakers control the inflation rate directly.[9]

Institutions

The president is elected for two periods by majority rule. The entire legislature is elected each period by strict proportionality.[10] These institutional characteristics, combined with the assumption that the growth shock persists for only one period, greatly facilitate the development of the formal model. Since presidents serve two periods, expectations about the results of midterm elections are relevant to on-year decisions; but voters never need to anticipate the effects of their decisions on the next presidential election. We can thus characterize the voting equilibrium in terms of a game with two moves, the simultaneous election of a president and Congress in on-years and the election of Congress at midterm. Split tickets are permitted on the first move. There is no abstention.

While economic and electoral outcomes will vary as a consequence of the random variables in the model, the equilibrium voting strategies used in each two-move game will be repeated indefinitely. Consequently, we use subscripts 0, 1, and 2 to denote elections occurring at the end of periods 0 and 1 and economic outcomes occurring in periods 1 and 2.

The Voters

There is a continuum of voters whose preferences are parameterized by $(\bar{\pi}^i, b^i)$. We assume that the distribution of voter preferences is not fully known, by anyone. This realistic feature of the model leaves electoral results uncertain even if the two parties' preferences and policies are common knowledge. Specifically, the inflation ideal points $\bar{\pi}^i$ are uniformly distributed, without loss of generality, on an interval of length 1. Now,

$$\bar{\pi}^i \sim U[\mathbf{a}, 1 + \mathbf{a}], \tag{5}$$

where **a** is a random variable, drawn independently[11] in every period from a uniform distribution on the temporally constant interval $[-w, w]$:[12]

$$\mathbf{a} \sim U[-w, w]. \tag{6}$$

We adopt the simplifying assumption that all voters have the same weight on growth b ($b^i = b$ for all i). In the Appendix, we give conditions where this assumption can be relaxed.

Rational Retrospective Voting and the Competency Model

Since voters benefit from growth (via b), they are inclined to retain in office an incumbent whom they believe to have greater-than-average competence. The key to the Persson and Tabellini (1990) model is that while the voters fully know the model of equations 1–3, they are missing one key piece of information, which is μ_t, the current period's contribution to the MA(1) competence process. They are assumed to know not only g_t, π_t, and π_t^e but also μ_{t-1}, the competency innovation of the previous period. With this information, their optimal forecast of the incumbent's competence in the period after the election is[13]

$$\hat{\eta}_{t+1} = [g_t - \bar{g} - \gamma(\pi_t - \pi_t^e) - \rho\mu_{t-1}]\rho \frac{\sigma_\mu^2}{\sigma_\zeta^2 + \sigma_\mu^2}; \tag{7}$$

that is, a rational retrospective voter's decision will be influenced only by growth net of the terms subtracted (average growth, $\bar{g}$, growth from unexpected inflation, $\gamma(\pi_t - \pi_t^e)$ and the portion of competence that does not carry over to the next term, $\rho\mu_{t-1}$. The term in square brackets is multiplied by ρ, the fraction of μ_t that gets carried over to the next period, and by a term that includes the variances. This last term allows the voter to reduce a forecast of future competence if the role of "luck" (ζ) in the economy is large relative to the role of "competence" (μ). In contrast to the rational retrospective voter, the naive retrospective voter keys on all of g_t.

The Timing of the Model

Schematically, the timing of events in our model is as follows:

Periods t = 0, 2, 4, . . .
Inflation (π_t) is determined by the government; shocks ζ_t, μ_t are assigned by "nature."
Output growth (g_t) is realized.
Binding economic plans (wage contracts) for period $t + 1$ are made by uncoordinated private agents.
The president (who serves in $t + 1$ and $t + 2$) and Congress (which serves in $t + 1$) are elected.

Periods t = 1, 3, 5 . . .
[Identical except that president remains in office and only Congress (which serves in $t + 1$) is elected.]

The Time-consistent Inflation Policies of the two Parties

A time consistency problem (Barro and Gordon 1983; Kydland and Prescott 1977) arises in this model. Each party would be better off if it could credibly commit to implementing, whenever it had control over policy, its inflation bliss point ($\bar{\pi}^D$ or $\bar{\pi}^R$). But a party in power, unable to resist the temptation to stimulate short-run growth through an inflation surprise, inflates up to the point at which the disutility of a surprise increment of inflation just offsets the resulting short-run output stimulation. Substituting equation 1 into equation 4 and taking first-order conditions yields the time-consistent inflation policies:

$$\pi_t^{D*} = \bar{\pi}^D + \gamma b^D > \pi_t^{R*} = \bar{\pi}^R + \gamma b^R \quad \forall t. \tag{8}$$

These inflation rates are higher than the corresponding party bliss points. But absent credible commitments to lower inflation rates, economic agents and voters anticipate the time-consistent rates, and parties implement them. Note that if voter i were dictator, his or her time-consistent policy would be

$$\pi_t^{i*} = \bar{\pi}^i + \gamma b \quad \forall t.$$

The Executive–Legislative Policy Interaction

Postelectoral inflation reflects the time-consistent policies of both parties, since policy is a function of which party holds the presidency and of the composition of Congress. The nature of the policy interaction between the legislature and the executive in practice is, of course, a complex question. We capture this interaction by the following expressions for the actual inflation rate when parties R and D hold the presidency, respectively:

$$\pi_t^R = \alpha\pi^{R*} + (1 - \alpha)[\pi^{D*}(1 - V_{t-1}^R) + \pi^{R*}V_{t-1}^R] \tag{9a}$$

and

$$\pi_t^D = \alpha\pi^{D*} + (1 - \alpha)[\pi^{D*}(1 - V_{t-1}^R) + \pi^{R*}V_{t-1}^R] \tag{9b}$$

where $0 < \alpha < 1$ and V_{t-1}^R is the Republican vote share in the congressional election at the end of period $t - 1$. These equations imply that the actual policy outcomes (π_t^D and π_t^R) are linear combinations of the president's policy (π^{D^*} or π^{R^*}) and the policy that would be pursued by an all-powerful Congress. The latter (given by the term in square brackets in the equations) is itself a linear combination where each party's time-consistent policy is weighted by its congressional vote share. The parameter α captures the relative weight of the president in policy formation.

Given that both parties are represented in Congress, the actual inflation rate will exceed the time-consistent policy of party R, π^{R^*} and fall short of that of party D, π^{D^*}. Conditional on the party of the president, the rate of inflation is increasing in the vote share for party D. Likewise, holding congressional vote shares constant, inflation is higher with a party D president than with a party R president:

$$\pi_t^D > \pi_t^R.$$

Analysis of the Electoral Model

In standard two-candidate voting models, since voters have only a binary choice, there is a unique voter equilibrium once weakly dominated strategies have been cast aside. In our model, only "extreme" voter types with indirect bliss points less than π^{R^*} (greater than π^{D^*}) have a weakly dominant strategy of always voting for party R (D).[14] More moderate voters do not have weakly dominant strategies. How they vote depends upon their conjectures about the behavior of other voters. Thus, there is a fundamental problem of coordination of voter strategies.

A plausible unique equilibrium can be characterized, however, by adapting the concept of coalition-proof Nash equilibria to this macroeconomic policy context (Bernheim, Peleg, and Whinston 1987).[15] The basic idea is that equilibrium strategies should be robust to "credible" defections of coalitions, as well as of individuals; that is, no "credible" coalition of voters would want to modify the electoral outcome by changing their votes.

Midterm Elections

At midterm, each voter has a single binary choice—vote D or R for Congress. This makes the equilibrium analysis very simple. With party R holding the presidency, there is a unique pivotal voter ideal point, $\tilde{\pi}^{CR}$. Any voter with $\tilde{\pi}^i < \tilde{\pi}^{CR}$ votes for party R, and those with $\tilde{\pi}^i > \tilde{\pi}^{CR}$ prefer D. The expected R congressional vote is

$$E(V_1^R) = \tilde{\pi}^{CR}. \tag{10}$$

The equilibrium-expected inflation level represents the indirect bliss point of the pivotal voter (Alesina and Rosenthal 1989):

$$E(\pi_2^R) = \tilde{\pi}^{CR} + \gamma b. \tag{11}$$

Suppose, to the contrary, that $E(\pi_2^R) < \tilde{\pi}^{CR} + \gamma b$. By continuity, voters with ideal points slightly less than $\tilde{\pi}^{CR} + \gamma b$ would also find that inflation was too low, and vote D to increase inflation, implying that $\tilde{\pi}^{CR}$ did not specify an equilibrium. A similar argument precludes $E(\pi_2^R) > \tilde{\pi}^{CR} + \gamma b$. Using equations 9–11, we find

$$\tilde{\pi}^{CR} = \frac{\pi^{R^*} - b\gamma + (1-\alpha)(\pi^{D^*} - \pi^{R^*})}{1 + (1-\alpha)(\pi^{D^*} - \pi^{R^*})}. \tag{12}$$

Analogous arguments show that when there is a D president at $t + 1$, there exists another cut-point, $\tilde{\pi}^{CD}$, given by

$$\tilde{\pi}^{CD} = \frac{\pi^{D^*} - b\gamma}{1 + (1-\alpha)(\pi^{D^*} - \pi^{R^*})}. \tag{13}$$

Equations 12 and 13 imply that the midterm vote for party D is increasing in b (which captures voters' tolerance for higher inflation in exchange for higher growth) regardless of the president's party. However, expected output is unaffected, since at higher values for b, economic agents correctly anticipate the higher inflation that ensues from D's higher congressional vote.

The Two-Period Model: President Unconstrained in Period 1

In the two-period model, each voter simultaneously makes two choices in on-years. Insight into the analysis is provided by decoupling these two choices and assuming, first, that a president unconstrained by Congress is elected in the first period.

For period 1, we now require an additional cut-point $\tilde{\pi}^P$. Individuals with indirect bliss points lower than $\tilde{\pi}^P$ will vote R for president, while those with higher bliss points will vote D. Thus, the expected vote for the Republican presidential candidate will be

$$E(V_0^{RP}) = \tilde{\pi}^P. \tag{14}$$

For an equilibrium to hold, a voter with ideal point $\tilde{\pi}^P$ must obtain the same expected utility from an R victory as from a D victory. This means that a voter at $\tilde{\pi}^P$ must be indifferent as between the two-period bundle of inflation and growth associated with the election of an R president and the bundle represented by a D president.

We have already computed the inflation outcomes in the bundles, namely

$$\left.\begin{array}{l}\text{If a } D \text{ president}\\ \text{(unconstrained in period 1)}\end{array}\right\} \begin{array}{lr} \pi_1^D = \pi^{D^*} & (15)\\ E(\pi_2^D) = \tilde{\pi}^{CD} + \gamma b & (16)\end{array}$$

$$\left.\begin{array}{l}\text{If an } R \text{ president}\\ \text{(unconstrained in period 1)}\end{array}\right\} \begin{array}{lr} \pi_1^R = \pi^{R^*} & (17)\\ E(\pi_2^R) = \tilde{\pi}^{CR} + \gamma b & (18)\end{array}$$

We also need to compute the expected growth outcomes in the bundles; in order to do so, we need to evaluate the period 0 rational expectation of inflation in period 1. This requires knowledge of the probability of a D presidential victory, given $\tilde{\pi}^P$. This probability, denoted $Q(\tilde{\pi}^P)$ can be easily computed, given the uniform distribution of **a**. The inflation expectation is then evaluated as

$$\pi_1^e = Q(\bar{\pi}^P)\pi^{D^*} + (1 - Q(\bar{\pi}^P))\pi^{R^*} \qquad \textbf{(19)}$$

Equation 19 is central to both wage contracting and voting. It underlines that before presidential elections, electoral uncertainty forces agents to "hedge their bets" in forming expectations about future policies. Using 19, we can now compute growth in the two periods. We assume a D president in period 0, the R case being symmetric. Expected growth is[16]

$$\left.\begin{array}{l}\text{if a } D\\ \text{president}\\ \text{at } t = 0\end{array}\right\}\begin{array}{ll} E(g_2^D) = E(g_2^R) = \bar{g} & \textbf{(20)}\\ E(g_1^R) = \bar{g} - Q(\bar{\pi}^{D^*} - \bar{\pi}^{R^*}) & \textbf{(21)}\\ E(g_1^D) = \bar{g} + (1 - Q)(\bar{\pi}^{D^*} - \bar{\pi}^{R^*}) + \hat{\eta}_1. & \textbf{(22)}\end{array}$$

Equation 20 implies that control of the White House will not affect growth in period 2. This result follows because even before the presidential election takes place, the voters know that new wage contracts will be signed at the end of period 1. At that time, there will be no uncertainty about the identity of the president.

Equation 21 gives the expected growth in the first period of an R administration. It is obtained by substituting equations 17 and 19 into equation 1 and by noting that expected competence is at its "normal" value of 0, as R is the challenging party, and so there is no information about its competence. Equation 21 shows that *below*-normal growth is expected at the outset of an R administration. This does not imply that R likes recessions. On the contrary, since $b^R > 0$, party R prefers higher growth to lower growth. The problem is that the possibility of a D electoral victory keeps inflation expectations (see equation 19) *higher* than the low-inflation policy of party R.

Equation 22 represents expected growth in period 1 when party D retains control of the White House. The equation is symmetric to equation 21 in that there is *above* normal growth from surprise inflation. The additional term, $\hat{\eta}_1$, is the expected value of the incumbent party's competence in period 1, from equation 7. (The expectation at $t = 0$ for period 2 is 0 because competence is MA(1).) This is the key to "rational" retrospective voting. Voters will tilt to incumbents with large $\hat{\eta}$s.

With equations 14–22, we have all the necessary information to compute $\bar{\pi}^P$ and $Q(\bar{\pi}^P)$ (see Appendix for details). It should be clear now that there are two influences on presidential voting. The first is how the parties are located relative to the distribution of voter preferences. If R's time-consistent policies are closer to the median indirect bliss point than are D's, R will be favored ($Q < 1/2$). The second is the rate of economic growth in the preelection period, via g's influence on $\hat{\eta}$. We emphasize that the relevant portion of growth reflects only "competence" and "luck" and not the "partisan" inflation policies of the two parties (see equation 7).

The Two-Period Model with Congress Elected in Both Periods

Now we must find one more cut-point $\bar{\pi}^C$, which applies to the on-year congressional elections. Again, voters with indirect bliss points below the cutpoint will vote for R, and those above will vote for D. As in the midterm case, first-period inflation equals, in expectation, the bliss point of the pivotal voter. Applying results from Alesina and Rosenthal (1991), we have

$$\bar{\pi}^C = Q(\bar{\pi}^P)\bar{\pi}^{CD} + (1 - Q(\bar{\pi}^P))\bar{\pi}^{CR}. \qquad \textbf{(23)}$$

The first-period congressional cutpoint is a weighted average of the second-period cut-points, with the weights given by the probability of the presidential election outcome. This equation implies that on-year congressional voting responds directly to future partisan effects on economic policy, via $\bar{\pi}^{CD}$ and $\bar{\pi}^{CR}$, but only indirectly to the current state of the economy, via the linkage to presidential voting provided by $Q(\bar{\pi}^P)$. There is no direct response because voters evaluate competence only for the executive.

A "midterm cycle" occurs if and only if $0 < Q(\bar{\pi}^P) < 1$, so that, from equation 23:

$$\bar{\pi}^{CR} < \bar{\pi}^C < \bar{\pi}^{CD}; \qquad \textbf{(24)}$$

that is, a midterm cycle results when there is uncertainty about the outcome of the presidential election, leading voters to hedge their bets in on-years. With presidential uncertainty resolved, voters in midterm elections "fully" moderate the presidential winner. For example, consider a voter who desires moderately high inflation, with $\bar{\pi}^i \in (\bar{\pi}^C, \bar{\pi}^{CD})$. If the R presidential candidate were to win, a strong R congressional vote would result in inflation far below the rate favored by voter i. Accordingly, in the on-year, this voter protects against a possible R presidential win by supporting the D congressional delegation. But if party D captures the White House, the risk of a party R win evaporates and at the ensuing midterm election, voter i supports the party R delegation to moderate the D president.

Equation 24 implies that the less unexpected the outcome of presidential elections, the smaller the size of the midterm effect. The amount of uncertainty depends upon $\bar{\pi}^P$, which, in turn, is a function of the parameters of the model. The value of this cut-point can again be obtained by equating the expected utility from an R presidential victory to that from a D victory, this time taking into account the presence of a legislative vote in the first period. One can then compute expectations over growth and, after substitution, the presidential cut-point. The expressions for the presidential and congressional cutpoints generate two equations in two unknowns that are otherwise functions only of the parameters of the model (π^D, π^R, γ, β, ρ, b, w, σ_ζ^2, and σ_μ^2).

Alesina and Rosenthal (1991) show how to solve this problem and demonstrate, for a large set of parameter values, the existence of a unique equilibrium (in a different parameterization) characterized by uncertain presidential elections ($0 < Q < 1$) and thus a midterm cycle.

ESTIMATION

The Sample

Our empirical analysis of the joint determination of economic growth and the results of national elections covers 1915–88. We began with 1915 because the previous year, 1914, was the beginning of a new financial regime, marked by the inception of the Federal Reserve and the collapse of the gold standard.[17] Furthermore, economic data before this date are of dubious quality, and the three-party presidential race of 1912 would pose estimation problems. Descriptive statistics and data sources are given in Appendix Table A-1. In the theoretical model, each period represents two calendar years, mimicking the intervals between national elections. The Appendix generalizes the model to permit periods of one year, as in the estimation.

The Growth Equation

The growth equation of our theoretical model reflects two potential channels of political influence on the economy. First, there is "surprise inflation" in the wake of every presidential election. To capture this effect empirically, we construct a "partisan effect" variable, pe_t, set equal to 1 during the second year of a Republican administration, -1 during the second year of a Democratic administration, and 0 otherwise. The coefficient on this variable should be negative. The second channel from politics to growth is the executive branch's competence at promoting economic performance. In the theoretical model, executive competence, which is not directly observable, evolves according to an MA(1) process. This model implies that successive residuals from the growth equation will be less highly correlated when there is a change of administration.

The specification of pe_t has three features deserving explanation. First, the variable is nonzero only in the *second* year of presidential terms. This reflects the view that real effects of monetary policy do not show up in output before 2–4 quarters.[18] Using quarterly data, Alesina (1988) shows that the postelectoral effects on the economy appear no sooner than 2 quarters after a presidential election, peak in about 5–6 quarters, and disappear by 10 quarters. Analogous results for several other countries are reported by Alesina and Roubini (1992). Second, with pe_t, the magnitude of the inflation surprise is equal across parties, whereas in the theory, the surprise (see equations 21–22) is greater for the party with the lower probability of winning.[19] Third, in addition to the presidential inflation surprise captured by pe_t, our theoretical model also allows for surprise inflation from the outcome of congressional elections. However, whereas the presidential outcome is discrete, the impact of congressional elections depends on deviations of V^R from its expected value. These deviations should be roughly of the magnitude of the small forecast errors from our House equations. In addition, presidential influence on policy is likely to exceed that of Congress (Hibbs 1987); that is, α is likely to be large, implying that the impact of congressional surprises is sufficiently small that we can simplify the empirical model by excluding them.

With the term ζ_t in equation 2, we modeled transitory effects on growth as random events. But military activities, especially wars, represent an obvious source of transitory effects that can be included in the analysis.[20] Define m_t to be the number of individuals in military service as of 30 June of year t and POP_t to be the population of the United States for the same year. Then the rate of military mobilization is given by

$$mm_t = (m_t - m_{t-1})/\text{POP}_t.$$

This variable highlights the beginnings and endings of wars, and it scales conflicts relative to one another. Including pe_t and mm_t and substituting into equation 1 from equations 2 and 3, our growth equation is

$$\begin{aligned} g_t &= \gamma_0 + \gamma_1 pe_t + \gamma_2 mm_t + \zeta_t \\ &\quad + \mu_t^R + \rho\mu_{t-1}^R \quad \text{if an } R \text{ president} \\ g_t &= \gamma_0 + \gamma_1 pe_t + \gamma_2 mm_t + \zeta_t \\ &\quad + \mu_t^D + \rho\mu_{t-1}^D \quad \text{if a } D \text{ president.} \end{aligned} \quad \textbf{(25)}$$

Let θ denote the covariance between the two parties' competency shocks:

$$\theta = \text{Cov}(\mu_t^R, \mu_t^D).$$

In our model, $\theta = 0$, while $\theta = \sigma_\mu^2$ in the standard MA(1) model. The two models are nested in a generalized growth equation with parameters γ_0, γ_1, γ_2, σ_ζ^2, σ_μ^2, ρ, and θ.

Nelson and Plosser (1982) show that the standard MA(1) model where $\theta = \sigma_\mu^2$, and $\sigma_\zeta^2 = 0$ is not rejected in favor of more complicated autoregressive moving average (ARMA) process models.[21] Thus, the standard MA(1) model adequately describes the annual growth series.[22] In contrast, our model implies $\theta = 0$ and does not restrict σ_ζ^2. The model implied by equation 25 is underidentified: we cannot recover ρ, σ_μ^2, σ_ζ^2, and θ without further structure. (See the Appendix for a more detailed discussion of this test.) However, we can estimate $c_0 = \rho\sigma_\mu^2$, and $c_1 = \rho\theta$, enabling us to test both the competency-based model, which implies H_0: $c_0 > c_1 = 0$, and the standard MA(1) model, which implies H_1: $c_0 = c_1$.

Our estimate of c_1 is 10.51, with a standard error of 3.98, leading to rejection of H_0 at all standard levels of significance. Is there evidence that $c_1 < c_0$—as we might see if the competency shocks for the two parties were positively (but imperfectly) correlated? The estimated value of c_0 is 7.85, *less* than c_1, providing no evidence against H_1 in favor of the alternative that $c_1 < c_0$. We thus *reject* the competence model. This implies that rational voters should not be retrospective—the rate of GNP growth in election years should not affect presidential elections. We return to this point when we discuss the presidential vote results.[23]

Our test of H_1 against the unrestricted model is more favorable, indicating acceptance at all standard significance levels.[24] We thus adopt the standard MA(1) model:

$$g_t = \gamma_0 + \gamma_1 pe_t + \gamma_2 mm_t + \rho\mu_{t-1} + \mu_t. \quad \textbf{(26)}$$

Purely to facilitate estimation of the full model, we allow for heteroscedasticity with separate variances for nonelection years, midterm election years, and years in which there is a presidential election: $\sigma^2_{\mu\mathcal{N}}$, $\sigma^2_{\mu\mathcal{M}}$, and $\sigma^2_{\mu\mathcal{P}}$ respectively.[25] Our estimates do not lead to rejection of the null hypothesis that these variances are equal.

Presidential Elections

For convenience in estimation, we specify, for both presidential and congressional elections, the dependent variables as *shares of the two-party vote for the party of the incumbent president.*

In our theoretical model, voters evaluate both party policies and presidential competence. But since the tests of the growth model provided no evidence that variations in competence are an important factor in growth, any evidence of retrospective voting on the economy will, within the context of our model, constitute a rejection of voter rationality.

Our theoretical model of presidential voting implies a cut-point $\bar{\pi}_P$ that may not be at the expected median. Consequently, one party may have an expected vote share greater than one-half. We capture the embodiment of policy preferences in the cut-point via r_t, which takes on a value of 1 if the incumbent is a Republican, and 0 otherwise.

However, we allow the cut-point implicit in r_t to be "adjusted" via v^{hm}_{t-2}, the share of the popular vote cast for the incumbent president's House delegation during the preceding midterm election. This variable can be proxying for several effects. First, the locations of the parties *relative* to the distribution of the voters may adjust slowly in time, whereas they are assumed to be constant in the theoretical model. Second, the independent preference shocks in the theoretical model are likely to be serially correlated in practice. Third, incumbency advantage in the House may directly improve chances of winning the presidency. None of these mechanisms is included (for reasons of tractability) in our theoretical model, but our results suggest they are empirically relevant.

The presidential voting equation also contains an additional disturbance φ^p_t that is orthogonal to the growth shocks and measures. This incorporates **a** in the theoretical model. "Rally 'round the flag" effects from wars perhaps represent a systematic short-run shift in the distribution of preferences. Consequently, we also include mm_t in the equation.

Last—and certainly not least—is retrospective voting on the economy. We test for this effect by including g_t in the regression. But to pursue our investigation of "rational" versus "naive" voting further, we break g_t into two components: μ_t, the contemporaneous shock, and $\hat{g}_t$, which is the expected growth rate based on the parameters of equation 26 and lagged shock (recall that $pe = 0$ in an election year):

$$\hat{g}_t = \gamma_0 + \gamma_2 mm_t + \rho\mu_{t-1}.$$

Our voting equation is, then,

$$v^p_t = \psi_0 + \psi_1 r_t + \psi_2 v^{hm}_{t-2} + \psi_3 mm_t + \psi_4 \hat{g}_t + \psi_5 \mu_t + \varphi^p_t. \quad \textbf{(27)}$$

Since we rejected the competence model, voter rationality would imply, in this model, that $\psi_4 = \psi_5 = 0$. Purely naive retrospective voting implies $\psi_4 = \psi_5 > 0$; that is, the electorate votes on the basis of GNP growth and does not even attempt to make any distinction between shocks to growth and predictable growth. For a naive voter, growth is growth: its pedigree does not matter.

Note that φ^p_t does not appear in equation 26. Consequently, equations 26–27 represent a recursive system. The recursive structure is preserved when we add congressional voting.[26] We also estimate a restricted presidential equation in which there is no direct "rally 'round the flag" effect:

$$v^p_t = \psi_0 + \psi_1 r_t + \psi_2 v^{hm}_{t-2} + \psi_4 \hat{g}_t + \psi_5 \mu_t + \varphi^p_t. \quad \textbf{(28)}$$

House Elections

For House elections, we distinguish between presidential election years and midterm contests. In both cases we include military mobilization, the lagged House vote, and the incumbent's party affiliation, the latter to allow congressional cut-points to differ from the median.

In presidential years, we allow for coattails (Calvert and Ferejohn 1983; Erikson 1990). Two avenues for coattails need to be considered.[27] First, our formal model includes a random preference shock, **a**, which affects both races and induces positive correlation between the presidential vote and the congressional vote. Following Kramer (1971), we allow for this effect by making the House vote dependent on the presidential vote shock φ^p_t. We expect a coefficient on this variable of less than 1, since the presidential shock may contain candidate-specific effects that are "outside" our formal model. Second, naive retrospective voting may induce positive coattails in congressional elections due to a "feel good" effect: some of the effect of economic performance on the presidential vote carries over to the benefit of the party's congressional delegation (Erikson 1990).

We test for positive or negative coattails based on

TABLE 1

Equation for Annual Growth Rate: Percentage of Real Gross National Product, 1915–1988

VARIABLE	COEFFICIENT	UNRESTRICTED[a]	SYSTEM RESTRICTED[b]
Constant	γ_0	3.254 (.728)	3.214 (.726)
Partisan effect	γ_1	−1.700 (.902)	−1.698 (.887)
Military mobilization	γ_2	3.027 (.516)	3.087 (.447)
Lagged growth shock	ρ	.518 (.101)	.484 (.122)
Type of year			
Nonelection year[c]	$\sigma^2_{\mu\mathcal{N}}$	13.503 (3.254)	—
Midterm election year[d]	$\sigma^2_{\mu\mathcal{M}}$	21.704 (7.453)	—
Presidential election year[e]	$\sigma^2_{\mu\mathcal{P}}$	19.377 (6.336)	—

Note: The restricted estimates are computed using Rothemberg's (1973) optimum minium distance technique, which does not produce fresh σ^2 estimates. Standard errors are in parentheses.

[a] Equation 26, estimated jointly with equations 27, 29, and 31.

[b] Equation 26, estimated jointly with equations 28, 30, and 32.

[c] $R^2 = .438$.

[d] $R^2 = .658$.

[e] $R^2 = .300$.

economic performance by estimating two versions of the presidential year house voting equation. In the first specification, there is no direct effect from the presidential vote. The instrumented growth rate and contemporaneous growth shock enter directly. Coattails arise solely from the shock to presidential voting:

$$v_t^{hp} = \lambda_0 + \lambda_1 r_t + \lambda_2 v_{t-2}^{hm} + \lambda_3 mm_t + \lambda_4 \hat{g}_t + \lambda_5 \mu_t + \lambda_6 \varphi_t^{p} + \varphi_t^{hp}. \quad (29)$$

In a second specification, neither the economy nor military mobilization nor the president's party affiliation matter once the effects of the shock to presidential preferences have been accounted for:

$$v_t^{hp} = \lambda_0 + \lambda_2 v_{t-2}^{hm} + \lambda_6 \varphi_t^{p} + \varphi_t^{hp}. \quad (30)$$

As for midterm elections, our theory predicts a consistent midterm backlash against the incumbent president's party as voters seek to moderate the policy impact of the incumbent president; formally, $v_t^{hm} < v_{t-2}^{hp}$. Two specifications are used. In the first, the midterm House vote depends on all variables that appear in the other equations (save for pe_t, which is perfectly collinear with r_t during midterm years):

$$v_t^{hm} = \kappa_0 + \kappa_1 r_t + \kappa_2 v_{t-2}^{hp} + \kappa_3 mm_t + \kappa_4 \hat{g}_t + \kappa_5 \mu_t + \varphi_t^{hm}. \quad (31)$$

A more extreme specification of the midterm cycle is that the seat loss is unaffected by anything other than the previous election's winning margin. This is embodied in our second specification of midterm voting:

$$v_t^{hm} = \kappa_0 + \kappa_2 v_{t-2}^{hp} + \varphi_t^{hm}. \quad (32)$$

Estimation Results

For each of the three voting equations in our model, we have just presented two versions, restricted and unrestricted. First, we jointly estimated the three unrestricted equations and the growth equation via maximum likelihood. The results appear in the first columns of Tables 1–4. We then estimated the restricted version of each voting equation, with the other equations unrestricted. These results are in the second columns of Tables 2–4. Finally, our preferred model, dubbed "system restricted", simultaneously restricts all three equations, with results in the last columns of Tables 1–4.

The estimates of the growth equation appear in Table 1. The partisan effect, the lagged growth shock and military mobilization are all significant at the 5% level. There are no statistically significant differences in the estimated variances, although the estimate for nonelection years is somewhat lower than the estimates for presidential and midterm years.

The estimated partisan effects parameter, γ_1, is of the same magnitude reported by Alesina and Sachs (1988) for a single-equation estimation covering 1948–84. It indicates that growth rates during the second year of Republican administrations with no changes

TABLE 2

Equation for Midterm Popular Vote for House of Representatives: Percentage of Two Party Vote for Incumbent President's Party, 1918–1986

		PARAMETER ESTIMATES		
VARIABLE	COEFFICIENT	UNRESTRICTED[a]	EQUATION RESTRICTED[b]	SYSTEM RESTRICTED[c]
Constant	κ_0	−1.398 (6.771)	−1.422 (6.230)	−1.418 (6.230)
Republican incumbent	κ_1	.240 (1.096)	—	—
Previous House vote	κ_2	.939 (.123)	.950 (.116)	.950 (.116)
Military mobilization	κ_3	−.369 (1.079)	— —	— —
Expected growth, $\hat{g}_t$	κ_4	.090 (.115)	—	—
Current growth shock	κ_5	.181 (.299)	—	—
Residual variance[d]	σ^2_{hm}	4.753 (1.586)	—	—

Note: The restricted estimates are computed using Rothemberg's (1973) optimum minimum distance technique, which does not produce fresh σ^2 estimates. Standard errors are in parentheses.

[a] Equation 31, estimated jointly with equations 26, 27, and 29.

[b] Equation 32, estimated jointly with equations 26, 27, and 29.

[c] Equation 32, estimated jointly with equations 26, 28, and 30.

[d] $R^2 = .853$.

in the level of the armed forces will average under 2%, while during the corresponding year of a Democratic administration, the economy will typically grow by almost 5%.

As expected, the beginnings and endings of wars overshadow other economic events. We estimate that the economy expands by about 3% for each 1% of the population that is mobilized into military service. When the same 1% are demobilized, the economy contracts, again by approximately 3%.[28]

Our estimate of ρ indicates that the effect of the lagged shock, μ_{t-1}, is approximately half the effect of the current shock, μ_t. This estimate is very similar to Nelson and Plosser's (1982) results.

Estimates for the unrestricted midterm House election equation (equation 31) appear in the first column of Table 2. This equation has an R-squared of .85. However, the predictive power of this equation stems almost entirely from the presence of the lagged House vote among the regressors. While no other explanatory variable is significant at the 10% level, the lagged House vote coefficients "t-ratio" is over 7. Equation 32 therefore imposes the restriction that the midterm House vote is influenced only by the preceding on-year vote. This results in a test statistic of $\chi^2_4 = 1.824$, corresponding to a *p*-value of .768, a "success" for our theoretical model in which the state of the economy does not affect House elections.

Estimates of equation 32 appear in the second column of Table 2. Routine calculations show that the predicted vote loss is 3.73% when the president's party won 46% (the sample minimum for victorious presidents) of the on-year House vote and increases to 4.54% at 62% (the sample maximum). Although the estimated intercept is insignificantly different from 0 and the coefficient of the lagged vote is insignificantly different from 1, the predicted vote loss is significant for the range of sample observations. For example, the party of an incumbent president that received 50% of the House vote in the preceding presidential election year is expected to lose 3.93%. The estimated standard deviation of this expected loss is only .65%. This result echoes Erikson (1990), who imposed $\kappa_2 = 1$ and used only postwar data.

To summarize our midterm House estimates, the systematic midterm effect is consistent with our theoretical prediction of a moderation of the president's party. Moreover, the fact that the midterm effect is increasing in the lagged vote for the president's party is consistent with regression to the mean induced by the random shock to preferences, **a**.

The first column of Table 3 displays the unrestricted presidential vote shares equation (equation 27). The economy has a pronounced effect on presidential voting. Both instrumented growth (ψ_4) and the current growth shock (ψ_5) have statistically significant coefficients. As we have discussed, the naive retrospective voting hypothesis has $\psi_4 = \psi_5$. Tests of this hypothesis indicate acceptance, whether one uses the unrestricted or system restricted estimates. In the former case, the asymptotic t-ratio is −.96,

TABLE 3

Equation for Popular Vote for President: Percentage of Two Party Vote for Incumbent President's Party, 1916–1988

VARIABLE	COEFFICIENT	PARAMETER ESTIMATES UNRESTRICTED[a]	 EQUATION RESTRICTED[b]	 SYSTEM RESTRICTED[c]
Constant	ψ_0	5.622 (10.410)	5.981 (10.147)	12.209 (9.470)
Republican incumbent	ψ_1	10.365 (1.929)	10.362 (1.919)	8.842 (1.223)
Previous House vote	ψ_2	.743 (.194)	.739 (.193)	.692 (.187)
Military mobilization	ψ_3	−.406 (2.358)	—	—
Expected growth, $\hat{g}_t$	ψ_4	1.636 (.509)	1.590 (.405)	.795 (.216)
Current growth shock	ψ_5	1.139 (.208)	1.140 (.208)	1.174 (.201)
Residual variance[d]	σ_p^2	13.601 (4.425)	—	—

Note: The restricted estimates are computed using Rothemberg's (1973) optimum minimum distance technique, which does not produce fresh σ^2 estimates. Standard errors are in parentheses.

[a] Equation 27, estimated jointly with equations 26, 29, and 31.

[b] Equation 28, estimated jointly with equaitons 26, 29, and 31.

[c] Equation 28, estimated jointly with equations 26, 30, and 32.

[d] $R^2 = .764$.

while in the latter it is 1.32. Both results are consistent with the hypothesis of naive retrospective voting. In fact, since the results show that voters do not treat components of growth differentially (though they might be expected to under rational retrospection), these results provide stronger support for naive retrospection than is available in previous single equation estimates. The effect of growth is substantial; in the system restricted estimation, a 1% increase in the election-year growth rate increases the vote for the incumbent president's party by .795%.[29] Even larger impacts arise in the other specifications.

The insignificant coefficient on the military mobilization variable in equation 27 shows that "rally 'round the flag" effects are manifest only through growth. The beginning and ending of wars, except insofar as they stimulate growth, are neutral for the incumbents' electoral fortunes. Estimates of the other coefficients change little when mm_t is deleted.

The estimated pro-Republican bias of 10% (ψ_1) is only partly counteracted by the effect of the lagged House vote, which favors the Democrats. Ceteris paribus, for a Democratic incumbent president to be more favored than a Republican, the Democrats would have to have done exceptionally well in the preceding midterm election, obtaining almost 57% of the House vote.

While our theoretical model allows for a partisan bias by voters, it requires that there be no bias to the incumbent party in presidential voting. This hypothesis is testable: the predicted Republican presidential vote in an election year with a Republican incumbent and the predicted Democratic presidential vote given a Democratic incumbent—in both cases with the explanatory variables at the sample mean—should sum to 100% of the two-party vote. If the sum is significantly greater, then there is a bias toward incumbents in addition to the pro-Republican bias. We test this in the context of the unrestricted model. The sum of the predicted incumbent totals is 105.59. With respect to the null hypothesis of 100, the t-ratio is 2.12, indicating rejection of the null hypothesis of no bias toward incumbents at the 5% level.

The presidential year House voting results appear in Table 4. As with midterm voting, the party affiliation of the president, military mobilization, and the growth variables have individually insignificant effects. The absence of a significant partisan bias in the congressional races coupled with the significant pro-Republican bias for the presidency shows that the Democrats are relatively more favored in the House than in presidential races, leading, other effects aside, to split-ticket voting.

The absence of a significant effect for growth is consistent with recent work by Erikson (1990), who included both the current presidential vote and the current growth rate on the right-hand side of a House voting equation and found that the coefficient of growth was insignificant. Our analysis confirms this result in a context that is free from the possible simultaneity bias of Erikson's estimator.

The presidential vote *shock* does have a highly

TABLE 4

Equation for Popular Vote for House of Representatives in Presidential Election Years: Percentage of Two Party Vote for Incumbent President's Party, 1916–1988

VARIABLE	COEFFICIENT	PARAMETER ESTIMATES UNRESTRICTED[a]	 EQUATION RESTRICTED[b]	 SYSTEM RESTRICTED[c]
Constant	λ_0	3.637 (7.424)	7.855 (6.492)	7.809 (6.497)
Republican incumbent	λ_1	1.345 (1.366)	—	—
Previous House vote	λ_2	.886 (.138)	.860 (.131)	.860 (.131)
Military mobilization	λ_3	1.567 (1.688)	—	—
Expected growth, $\hat{g}_t$	λ_4	.554 (.370)	—	—
Current growth shock	λ_5	−.032 (.153)	—	—
Presidential vote shock	λ_6	.547 (.105)	.534 (.065)	.531 (.065)
Residual variance[d]	σ^2_{hp}	2.841 (.937)	—	—

Note: The restricted estimates are computed using Rothemberg's (1973) optimum minimum distance technique, which does not produce fresh σ^2 estimates. Standard errors are in parentheses.

[a] Equation 29, estimated jointly with equations 26, 27, and 31.

[b] Equation 30, estimated jointly with equations 26, 27, and 31.

[c] Equation 30, estimated jointly with equations 26, 28, and 32.

[d] $R^2 = .905$.

significant effect on the on-year House vote, with a one-percentage-point shock to the presidential vote translating into approximately half an extra percentage point in the popular vote for the president's House delegation. This is consistent with the view that observed coattails result from a shock to preferences that shifts more voters to the same side of *both* the presidential and House voting cut-points.

When the insignificant variables from the on-year House voting equation are simultaneously dropped, the resulting χ_4^2 statistic of 6.63 indicates acceptance at all standard significance levels. The restricted presidential-year and midterm-year House voting equations have very similar structures, save for the effects of the presidential vote shock in the on-year equation. However, while the midterm voting equation reflects a systematic bias against the president's House delegation, we find no evidence of such bias in the on-year House elections. Holding the presidential vote shock equal to zero, and setting all other variables at their sample means, routine calculations reveal a statistically insignificant expected vote *gain* of about 1 percentage point for the president's party in a presidential year House election. The estimates do reveal a slight—but statistically insignificant—tendency toward mean reversion, with an expected vote gain of 2.08 percentage points with the lagged House vote at the sample minimum of 41% and a loss of .29 percentage points with a lagged House vote of 58%, the sample maximum for presidential election years.

Our preferred model consists of the system restricted set of equations—equations 26, 28, 30, and 32—whose jointly estimated values appear in the right-hand columns of Tables 1–4. To explore the dynamics of the system, we use our coefficient estimates to simulate the system in the absence of any random shocks to either growth or voting behavior and with military mobilization set to zero. Regardless of the starting value for the previous House vote, the system converges to an 36-year cycle, with the White House changing hands at regular intervals (see Table 5).

The cycling results from the cumulative effect of midterm losses. Each midterm loss costs the party of the incumbent a larger share of the vote than it wins back through mean reversion in the ensuing presidential election two years later. Indeed, when the president's House delegation is sufficiently large, small additional losses through mean reversion are expected in the on-year election. The longer a party retains control of the White House, the greater the cumulative erosion of its congressional delegation. Because the presidential vote is an increasing function of the lagged House vote, erosion of support for the incumbent's House delegation reduces its presidential vote. This process eventually costs the incumbent's party the White House.

The marked partisan bias toward Republican presidential candidates (which we have discussed) results in the Republican party's retaining control of the

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TABLE 5

Long-Run Stable Cycle for the System

YEAR	GROWTH RATE	VOTE FOR INC. PRES. PARTY		ELECTION WINNER	
		PRESIDENT	HOUSE	PRESIDENT	HOUSE
1	3.214	—	—	—	—
2	4.912	—	55.08	—	D
3	3.214	—	—	—	—
4	3.214	52.88	55.18	D	D
5	3.214	—	—	—	—
6	4.912	—	51.00	—	D
7	3.214	—	—	—	—
8	3.214	50.06	51.67	D	D
9	3.214	—	—	—	—
10	4.912	—	47.67	—	R
11	3.214	—	—	—	—
12	3.214	47.75	48.81	R	R
13	3.214	—	—	—	—
14	1.516	—	47.22	—	D
15	3.214	—	—	—	—
16	3.214	56.28	48.41	R	D
17	3.214	—	—	—	—
18	1.516	—	44.58	—	D
19	3.214	—	—	—	—
20	3.214	54.45	46.14	R	D
21	3.214	—	—	—	—
22	1.516	—	42.42	—	D
23	3.214	—	—	—	—
24	3.214	52.96	44.29	R	D
25	3.214	—	—	—	—
26	1.516	—	40.66	—	D
27	3.214	—	—	—	—
28	3.214	51.74	42.77	R	D
29	3.214	—	—	—	—
30	1.516	—	39.22	—	D
31	3.214	—	—	—	—
32	3.214	50.74	41.54	R	D
33	3.214	—	—	—	—
34	1.516	—	38.04	—	D
35	3.214	—	—	—	—
36	3.214	49.93	40.52	D	D

Note: The system simulated is

$$
\begin{array}{rlll}
g_t = & 3.214 - 1.698pe_t & & t = 0, 1, 2, 3, 4, \ldots \\
V^p_t = & 12.209 + 8.842r_t & + .692V^{hm}_{t-2} + .795g_t & t = 2, 6, 10, 14, \ldots \\
V^{hp}_t = & 7.809 & + .860V^{hm}_{t-2} & t = 2, 6, 10, 14, \ldots \\
V^{hm}_t = & -1.418 & + .950V^{hp}_{t-2} & t = 0, 4, 8, 12, \ldots
\end{array}
$$

Starting values must be assigned to r_t and V^{hp}_{t-2}. The steady state is reached from all starting values assigned. See Table A-1 and text for definitions of variables.

White House for 24 of the 36 years of the cycle. However, Republican control of the White House typically occurs with a divided government. There is unified Republican control of the executive and legislative branches for only 2 of the 36 years. Although the Democratic party only controls the White House for 12 years of the cycle, it receives a majority of the House vote during 10 of these years. Thus, to the extent that unified government is important to policy initiatives, the Democrats may actually have more opportunities to implement new policies than the Republicans.

The pattern of divided government, with Republicans occupying the White House and Democrats entrenched in Congress, is similar to actual post–World War II experience. However, it does not resemble the political climate of the 1930s, which was dominated by the "shock" of the Great Depression.

CONCLUSION

We have tested, in a fully simultaneous estimation, a macro model of economic growth and national elections. This model incorporates (1–2) the "rational partisan model" of growth; (3) voters' moderating behavior, which counterbalances the president via the congressional vote both in on-years via split tickets and in midterm elections; and (4) the "competency" model of rational retrospective voting. We found strong support for features 1–2 and 3 and rejected feature 4. Rather than summarizing these results in detail, we conclude by highlighting some open issues.

There are two inherent limitations to our enterprise. On the theoretical side, formulating a tractable model has forced us to limit dynamic considerations to the anticipation of midterm elections in years when the presidency is at stake; that is, with respect to the strategies pursued by political and economic agents, "the world starts over" every four years. Our finding of a bias in favor of presidential incumbents suggests an interest in developing a model with a longer time horizon, where voters are risk-averse with regard to the growth rate. On the empirical side, data are thin. In the model, only presidential elections have an important impact on the economy; and they occur only every four years. While output is produced continuously, persistence in the time series limits the information available for testing. In addition, the presence of only seven shifts in party control of the presidency since 1915 makes it difficult to distinguish persistence in administrative competence from other forms of persistence in the economy.

Even with these limitations in mind, our results are not good news for the attempt to explain retrospective voting on the economy via rational choice models. Shocks to the economy appear to be short-lived and unlinked to changes in partisan control of the White House. The analysis of growth gives no evidence that voters *should* use information about aggregate growth to learn about competence. Nevertheless, the effects of the economy on voting are consistent with naive retrospective voting.

A further challenge to rational choice models might result if we were to include measures of the probability of victory in our econometric model. While elections that are several months distant may always appear uncertain to economic agents, many postwar elections were known landslides on election eve. In such cases, we should not, according to our theoretical model, observe a midterm effect. But the midterm cycle is uniformly present. Discriminating true failures of the model from changes in preferences is, of course, difficult. While Nixon's 1972 election may have been "certain," Watergate intervened to produce a pro-Democratic shift at midterm. Even if it were possible to measure either probabilities or preference shifts, an expansion of the econometric model would further tax degrees of freedom. Nonetheless, the pervasiveness of the midterm cycle may attest to voters treating probabilities differently than in standard rational choice models.[30] Consequently, an explicit treatment of voter expectations just prior to voting is a strong candidate for future research.

In short, we have presented a unified rational choice model of national elections and the macroeconomy. The empirical tests of the model led to rejection of rational retrospective voting but to strengthened support for rational responses to electoral uncertainty via both the midterm electoral cycle and the partisan business cycle. To reconcile these contrasting results on rationality would be an excellent aim for future theoretical and empirical research.

APPENDIX

Heterogeneity in *b*

Alesina and Rosenthal (1991) characterized the Coalition Proof Nash (CPN) equilibria in a general one-dimensional voting model with two polarized parties and the same institutional structure used herein. With a common b to all voters, our model here is also one-dimensional. It is straightforward to redefine variables and apply the results in Alesina and Rosenthal 1991. The indirect bliss points given in the text play the role of the one-dimensional bliss points in Alesina and Rosenthal 1991. The additional technical problem introduced herein is provided by the second dimension of preference parameters represented by b^i.

Equilibrium at t = 1 with heterogeneous b. Alesina and Rosenthal (1991) showed that the midterm equilibrium characterized by equations 12 and 13 not only is CPN but also Strong Nash. Here, we show how to generalize this result when the b^i differ across voters. Using equations 1 and 4 in the text, voter i maximizes:

$$E\left(-\frac{1}{2}(\pi_2-\bar{\pi}_i)^2+b^i[\bar{g}+\gamma(\pi_2-\pi_2^e)+\varepsilon_t]\right).$$

While the party of the president for the second period is known at the midterm elections, second-period inflation is treated as a random variable by the voters, since actual inflation will reflect the outcome of the midterm elections. We use the notation $\pi_2(V_1^D)$ to express this dependence, where $V_1^D = 1 - V_1^R$. The growth shock ε, long-term growth parameter $\bar{g}$, and expected inflation rate π_2^e are all unaffected by the voting decisions. Consequently, maximization of expected utility is equivalent to maximization of

$$W^{*i}=-\frac{1}{2}E[(\pi_2(V_1^D)-\bar{\pi}_i)^2]+b^i\gamma E[\pi_2(V_1^D)].$$

Without loss of generality, we assume an R president, the D president case being symmetric. We also assume the following:

ASSUMPTION 1. $b_i \sim f(b_i)$, $E(b_i) = b$, $\bar{\pi}^i \mid b_i \sim U[\mathbf{a}, 1+\mathbf{a}]$.

In other words, for every b^i, voter preferences on inflation have the same uniform distribution. (The marginal density of b^i is unrestricted.)

Using equation 8, we write,

$$\pi_2^R = \pi^{R^*} + KV_1^D; \quad K = (1-\alpha)(\pi^{D^*} - \pi^{R^*}).$$

After substitution and some algebra, we have

TABLE A-1

Descriptive Statistics, 1915–88

VARIABLE	MEAN	S.D.	N
Growth rate of real GNP (g)	3.062	5.981	74
Nonelection year growth rate	3.610	4.967	37
Midterm year growth rate	1.528	8.192	18
Presidental year growth rate	3.447	5.404	19
Party (D = 0, R = 1) of president (r)	.486	.503	74
Presidential vote for incumbent's party (V^p)	53.053	7.805	19
On-year House vote for incumbent's party (V^{hp})	49.907	5.610	19
Lagged House vote (on-year)	49.279	4.790	19
Midterm House vote for incumbent's party (V^{hm})	49.220	4.922	18
Lagged House vote (at midterm)	53.320	4.551	18
Partisan effect (pe) in 2d year of term	.000	1.029	18
Military mobilization (mm)	.010	1.007	74

Sources: Voting data from U.S. Bureau of the Census, *Historical Statistics of the United States, Colonial Times to 1970 Bicentennial edition, part 2 (Washington: GPO, 1975); idem, Statistical Abstract of the United States 1990,* 110th ed. (Washington: GPO, 1990). Erikson 1989, 1990; Real GNP data from Balke and Gordon 1989 and CITIBASE. The Balke and Gordon series is similar to that of Romer (1989).

$$W^{*i} = -\frac{1}{2}K^2[E(V_1^D)]^2 + (\bar{\pi}^i + b^i\gamma - \pi^{R*})KE(V_1^D)$$

$$+ K^2\mathrm{Var}(V_1^D) + \text{constants.}$$

The assumption in n. 14 guarantees that $\mathrm{Var}(V_1^D)$ is constant for all voter strategies and thus for all $E(V_1^D)$ (see Alesina and Rosenthal 1991, Prop. 6, Proof). It follows, using the first-order condition, that there is a unique value of $E(V_1^D)$ that maximizes expected utility given by

$$E(V_1^D)^{*i} = \frac{\bar{\pi}^i + b^i\gamma - \pi^{R*}}{K}.$$

Using equation 8, we can verify that the maximizing expected vote level leads to expected inflation equal to the time-consistent inflation policy that would be pursued by a voter–dictator:

$$E(\pi_2^R)^{*i} = \bar{\pi}^i + b^i\gamma.$$

It can also be seen that there is a linear $(\bar{\pi}_i, b_i)$ locus that describes voters with identical preferences. Assume that a voter with preferences $\bar{\pi}^{CR} + b\gamma$ is at his or her maximum. Then, Strong Nash implies that voters with "lower" indirect preferences vote R and voters with "higher" preferences vote D (Alesina and Rosenthal 1991). Consequently, voter types with growth preference b^i vote D with probability $1 + \mathbf{a} - \bar{\pi}^{CR} - b\gamma + b^i\gamma$. We thus have

$$V_1^D = \int_{b_L}^{b_U} (1 + \mathbf{a} - \bar{\pi}^{CR} - b\gamma + b^i\gamma) f(b^i)\,db^i$$

$$= 1 + \mathbf{a} - \bar{\pi}^{CR}, \quad \text{where } E(V_1^D) = 1 - \bar{\pi}^{CR}. \tag{A-1}$$

More generally, assume that $(\bar{\pi}^i, b^i)$ are drawn from some density $f(\bar{\pi}^i, b^i)$, such that the marginal density of $\bar{\pi}^i$ is uniform. If a voter with preferences $\bar{\pi}^{CR} + b\gamma$—where b need not equal $E(b^i)$—is at his or her maximum, then we assume the following:

ASSUMPTION 2. $V_1^D = \displaystyle\int_{\mathbf{a}}^{1+\mathbf{a}} \int_{\frac{\bar{\pi}^{CR} + b\gamma - \bar{\pi}^i}{\gamma}}^{b^U} f(b^i \mid \bar{\pi}^i)\,db^i\,d\bar{\pi}^i$

and $V_1^D = 1 + \mathbf{a} - \bar{\pi}^{CR}$ *if*

$$\int_{\mathbf{a}}^{\bar{\pi}^{CR}} \int_{\frac{\bar{\pi}^{CR} + b\gamma - \bar{\pi}^i}{\gamma}}^{b_U} f(b^i \mid \bar{\pi}^i)\,db^i\,d\bar{\pi}^i =$$

$$\int_{\bar{\pi}^{CR}}^{1+\mathbf{a}} \int_{b_L}^{\frac{\bar{\pi}^{CR} + b\gamma - \bar{\pi}^i}{\gamma}} f(b^i \mid \bar{\pi}^i)\,db^i\,d\bar{\pi}^i$$

Assumption 2 says that voters with $\bar{\pi}^i > \bar{\pi}^{CR}$ who vote R are exactly offset by voters with $\bar{\pi}^i < \bar{\pi}^{CR}$ who vote D. (This is similar to conditions of radial symmetry or median in all directions that appear in spatial voting theory [Enelow and Hinich 1984].) The assumption is different from these conditions in that it applies to a pivotal voter type, rather than a median. It is also weaker in that it applies, through integration, only to an aggregated property of the distribution of voter types. Assumption 1 implies Assumption 2. The assumption in n. 14 guarantees that if Assumption 2 holds for any $\mathbf{a}$, it holds for all $\mathbf{a}$. Thus, if we find that there exists a $\bar{\pi}^{CR}$ such that equation 12 holds and Assumption 2 is valid at $\bar{\pi}^{CR}$, then equation A-1 holds. Thus, independence is sufficient, but not necessary, for equation A-1.

Equation A-1 can also hold when, rather than having a conditional density given $\bar{\pi}^i$, b^i is functionally dependent on $\bar{\pi}^i$. Assume that a voter with preferences $\bar{\pi}^{CR} + b^i(\bar{\pi}^{CR})\gamma$ is at his or her maximum. This leads directly to the following assumption:

ASSUMPTION 3.

If $\bar{\pi}^i > \bar{\pi}^{CR}$, $\bar{\pi}^i + b^i(\bar{\pi}^i)\gamma > \bar{\pi}^{CR} + b^i(\bar{\pi}^{CR})\gamma$

and if $\bar{\pi}^i < \bar{\pi}^{CR}$, $\bar{\pi}^i + b^i(\bar{\pi}^i)\gamma < \bar{\pi}^{CR} + b^i(\bar{\pi}^{CR})\gamma$.

Assumption 3 holds when b_i is a strictly increasing function of $\bar{\pi}_i$. Therefore, equation A-1 holds for two polar cases of heterogeneity of b_i: independence and perfect correlation. Showing that equation 12 in the main text defines a Strong Nash equilibrium follows (with appropriate redefinition of variables) from the proof in Alesina and Rosenthal 1991.

The Two-Period Voting Equilibrium with Heterogeneous b^i *and President Unconstrained in Period 1.* Assume that analogs to Assumption 2 or 3 also hold, with $\bar{\pi}^P$ substituted for $\bar{\pi}^{CR}$. The two-period indifference conditions in Alesina and Rosenthal 1991 can be used to find the indifference condition for a voter type $\bar{\pi}^i$, b^i. (See also equation A-4.) As with *equilibrium at* $t = 1$ *with heterogeneous* b, the locus of indifferent types is linear. The remainder of the development follows the previous section.

The Full Model with Heterogeneous Growth Preferences. The setup again directly parallels that of *equilibrium at* $t = 1$ *with heterogeneous* b. It is straightforward to show that the relevant loci are again linear.

A General Annualized Model

We shall recast the model as an annual model with elections occurring only at the end of even periods, with on-year elections in periods 0, 4, etc. and midterm elections in periods 2, 6, etc. The policy π depends only upon the identity of the president and the vote in the previous congressional election. Thus, the policy is constant for two periods. We use the notation $\pi^J_{12}(V^D_0)$ to denote the policy for the first two periods when party J won the presidential election in period 0 and the congressional vote for D was V^D_0. Similarly, $\pi^J_{34}(V^D_2)$ gives the policy for periods 3 and 4. Except where noted, the specification of the model is unchanged from the text. The generalization of the model assumes that the competency shock follows an MA(4) process:

$$\eta_t = \mu_t + \sum_{k=1}^{4} \rho_k \mu_{t-k}$$

$$0 \le \rho_k \le 1; \quad \mu_t \text{ is independently and identically distributed}; \quad E(\mu_t) = 0$$

$$\mu_t \begin{cases} \mu^D_t & \text{if } D \text{ president at } t \\ \mu^R_t & \text{if } R \text{ president at } t \end{cases} \qquad \textbf{(A-2)}$$

By equation A-2, any competency shocks that arrive in a four-year presidential term will not carry over beyond the next term. In addition, Congress is entirely elected every two years. Consequently, we can analyze the model as a four-period model where elections occur at the ends of periods 0 and 2 and realizations to utility occur in periods 1, 2, 3, and 4.

In midterm elections, therefore, utility-maximizing voters care only about the effect of their vote on the $t = 3$ and $t = 4$ components of their additive utility functions. Hence, the relevant objective function becomes

$$W^{*i} = -\frac{1}{2}(1+\beta)(E[(\pi^J_{34}(V^D_2) - \bar{\pi}_i)^2] + b^i \gamma E[\pi^J_{34}(V^D_2)]) \qquad \textbf{(A-3)}$$

Using equation A-3, it is straightforward to show that equations 12 and 13 continue to give the equilibrium for midterm elections.

We now turn to the on-year elections. Without loss of generality, assume there is a party D president in period 0. The indifference condition for presidential voting is

$$-\frac{1}{2}(1+\beta)(\pi^{D^*} - \bar{\pi}^P)^2 - \frac{\beta^2}{2}(1+\beta) \cdot [\mathrm{Var}(\pi^D_{34}) + (\bar{\pi}^{CD} + \gamma b - \bar{\pi}^P)^2] + \gamma b\Big(\sum_{k=1}^{4} \beta^{k-1} E(g^D_k)\Big) = -\frac{1}{2}(1+\beta)(\pi^{R^*} - \bar{\pi}^P)^2 - \frac{\beta^2}{2}(1+\beta) \cdot [\mathrm{Var}(\pi^R_{34}) + (\pi^{CR} + \gamma b - \bar{\pi}^P)^2] + \gamma b\Big(\sum_{k=1}^{4} \beta^{k-1} E(g^R_k)\Big). \qquad \textbf{(A-4)}$$

The growth expectations are given by

$$E(g^D_4) = \bar{g} + \rho^*_4 \mu^*_0$$

$$E(g^D_3) = \bar{g} + \rho^*_3 \mu^*_0 + \rho_4 \mu_{-1}$$

$$E(g^R_t) = \bar{g} \qquad t \in \{3, 4\}$$

$$E(g^D_2) = \bar{g} + (1 - Q)(\bar{\pi}^{D^*} - \bar{\pi}^{R^*}) + \rho^*_2 \mu^*_0 + \rho_3 \mu_{-1} + \rho_4 \mu_{-2}$$

$$E(g^D_1) = \bar{g} + (1 - Q)(\bar{\pi}^{D^*} - \bar{\pi}^{R^*}) + \rho^*_1 \mu^*_0 + \rho_2 \mu_{-1} + \rho_3 \mu_{-2} + \rho_4 \mu_{-3}$$

$$E(g^R_t) = \bar{g} + Q(\bar{\pi}^{D^*} - \bar{\pi}^{R^*}) \qquad t \in \{1, 2\},$$

where

$$\rho^*_k = \rho_k \frac{\sigma^2_\mu}{\sigma^2_\mu + \sigma^2_\zeta}.$$

Solving for the presidential cutpoint yields

$$\bar{\pi}^P = \{((\pi^{D^*})^2 - (\pi^{R^*})^2) + \beta^2((\bar{\pi}^{CD} + \gamma b)^2 - (\bar{\pi}^{CR} + \gamma b)^2) - \gamma b(2(\pi^{D^*} - \pi^{R^*}) + (1+\beta)\vartheta)\} / \{2(\pi^{D^*} - \pi^{R^*}) + 2\beta^2(\bar{\pi}^{CD} - \bar{\pi}^{CR})\}$$

where

$$\vartheta = \sum_{k=1}^{4} \beta^{k-1} \rho^*_k \mu^*_0 + \sum_{k=2}^{4} \beta^{k-2} \rho_k \mu_{-1} + \sum_{k=3}^{4} \beta^{k-3} \rho_k \mu_{-2} + \rho_4 \mu_{-3}.$$

TABLE A-2

Single Equation Estimates of Annual Growth Rate (%) of Real GNP

		NESTED MODELS		
VARIABLE	COEFFICIENT	UNRESTRICTED (1)	$v_0 = v_1$ (2)	$c_0 = c_1$ AND $v_0 = v_1$ (3)
Constant	γ_0	3.100	3.081	3.047
		(.700)	(.705)	(.709)
Partisan effect	γ_1	−1.806	−1.862	−1.900
		(.851)	(.875)	(.850)
Military mobilization	γ_2	3.147	3.136	3.090
		(.506)	(.502)	(.489)
Same party variance	v_0	20.046	20.825	20.981
		(4.171)	(3.825)	(3.870)
Turnover variance	v_1	11.761	—	—
		(3.832)		
Same party covariance	c_0	8.356	7.853	8.260
		(2.504)	(2.230)	(2.276)
Turnover covariance	c_1	8.301	10.512	—
		(2.789)	(3.981)	
log likelihood		−208.139	−209.673	−209.874

Note: Standard errors are in parentheses.

This equation shows that the qualitative conclusions of the model are maintained. First, the incumbent's vote is nondecreasing in any competency shock the incumbent receives throughout his tenure. Second, the incumbent's vote is nondecreasing in the growth rate for the election year.

The model also continues to hold in the case where the president and the Congress are both elected in the first period. Because (1) growth shocks are unaffected by the outcome of congressional elections and (2) the policy π will be constant in periods 1 and 2, equation 23 continues to hold, and a presidential cutpoint equation can then be developed that is qualitatively similar to the equation for the MA(1) model.

Estimation

We shall offer further details of our test of the competency-based model. In addition, we shall allow for the possibility that the degree of persistence of competency shocks is different when there is a change of administrations, so that $\eta_t = \zeta_t + \mu_t^j + \rho_1 \mu_{t-1}^j$, with j designating the party of the new administration and where ρ_1 may differ from ρ. This leads us to a stand-alone growth model that depends on eight parameters: γ_0, γ_1, γ_2, σ_ζ^2, σ_μ^2, ρ, ρ_1, and θ. The model still suffers from underidentification: we can recover seven parameters: γ_0, γ_1, γ_2, v_0, v_1, c_0, and c_1. The parameters v_0, c_0, and c_1 are as defined in the text, while $v_1 = \sigma_\zeta^2 + \sigma_\mu^2 + \rho_1\sigma_\mu^2$. Estimates of this seven-parameter growth model appear in column 1 of Table A-2.

To test whether $\rho_1 = \rho$, it is sufficient to test H_2: $v_0 = v_1$. When we impose the restriction that $v_0 = v_1$, we obtain the estimates appearing in column 2 of Table A-2. This leads to a likelihood ratio test statistic of 3.07 (which is asymptotically distributed as χ_1^2), below the $\alpha = .05$ critical value of 3.84; thus, we accept H_2. In both columns 1 and 2, the coefficient of c_1 is positive and significant at all standard significance levels, leading to rejection of the competency model, which implies $c_1 = 0$.

The MA(1) model implies H_1: $v_0 = v_1$, $c_0 = c_1$. Parameter estimates obtained imposing this restriction appear in column 3 of Table A-2. The implied likelihood ratio statistic of 3.47 (which is asymptotically distributed as χ_2^2) is well below the $\alpha = .05$ critical value of 5.99. We thus conclude that there is no empirical basis for Persson–Tabellini rational retrospection, while we find no evidence against the standard MA(1) model in favor of a model with $\theta < 1$. Partisan differences in competence do not significantly affect economic growth.

Note that the coefficient estimates in column 3 for the partisan effects variable and for military mobilization differ slightly from those reported in Table 2. This difference arises not only from the simultaneous estimation but also from allowing separate residual variances for growth in years with elections. As indicated in the text, we cannot reject the hypothesis that the variances are the same. However, allowing them to differ makes simultaneous estimation of the growth and voting equations substantially more straightforward.

Maximum Likelihood Estimation of the Model

First, it is convenient to develop the following notation. We let $\mathcal{P}$ signify the set of all presidential election years in our sample, $\mathcal{M}$ the set of all midterm election years, and $\mathcal{N}$ the set of nonelection years: $\mathcal{P} = \{1916, 1920, \ldots, 1988\}$, $\mathcal{M} = \{1918, 1922, \ldots, 1986\}$, and $\mathcal{N} = \{1915, 1917, 1919, \ldots, 1987\}$. Let $P =$ the number of elements in $\mathcal{P}$, $M =$ the number of elements in $\mathcal{M}$, and $N =$ the number of elements in $\mathcal{N}$, while $T = \mathrm{P} + \mathrm{M} + \mathrm{N}$. We let h_t denote the set of errors and the explanatory variables realized before year t: $h_t \equiv \{\mu_s, \varepsilon_s^p, \varepsilon_s^{hp}, \varepsilon_s^{hm}, mm_s, v_{1914}^{hm}, r_{1914} | s < t\}$. Denote by v the vector of all shocks in our model: $\{\mu_t\}_{t=1915}^{1988}, \{\varepsilon_t^p\}_{t \in \mathcal{P}}, \{\varepsilon^{hp}\}_{t \in \mathcal{P}}, \{\varepsilon^{hm}\}_{t \in \mathcal{M}}$. The shocks are independently normally distributed with joint density $f(v)$. Using repeated application of Bayes' Rule, we can rewrite this joint density as

$$f(v) = g_p(\mu_{1988}, \varepsilon_{1988}^p, \varepsilon_{1988}^{hp} | h_{1988}) \cdot g_n(\mu_{1987} | h_{1987})$$

$$\cdot\, g_m(\mu_{1986}, \varepsilon_{1986} | h_{1986}) \cdot \ldots \cdot g_n(\mu_{1915} | h_{1915}).$$

Taking logs, we obtain

$$ln(f(v)) = \sum_{t \in \mathcal{P}} g_p^*(\mu_t, \varepsilon_t^p, \varepsilon_t^{hp} | h_t)$$

$$+ \sum_{t \in \mathcal{N}} g_n^*(\mu_t | h_t) + \sum_{t \in \mathcal{M}} g_m^*(\mu_t, \varepsilon_t | h_t),$$

where

$$g_n^*(\mu_t | h_t) = -\frac{1}{2} ln(2\pi) - \frac{1}{2} ln(\sigma_{\mu\mathcal{N}}^2) - \frac{\mu_t^2}{2\sigma_{\mu\mathcal{N}}^2}$$

$$g_m^*(\mu_t, \varepsilon_t^{hm} | h_t) = -ln(2\pi) - \frac{1}{2} ln \begin{vmatrix} \sigma_{\eta\mathcal{M}}^2 & 0 \\ 0 & \sigma_{hm}^2 \end{vmatrix}$$

$$- \frac{1}{2} [\mu_t, \varepsilon_t^{hm}] \begin{bmatrix} \sigma_{\mu\mathcal{M}}^2 & 0 \\ 0 & \sigma_{hm}^2 \end{bmatrix}^{-1} \begin{bmatrix} \mu_t \\ \varepsilon_t^{hm} \end{bmatrix}$$

$$= -\frac{1}{2} ln(2\pi) - \frac{1}{2} ln(\sigma_{\mu\mathcal{M}}^2) - \frac{\mu_t^2}{2\sigma_{\mu\mathcal{M}}^2}$$

$$- \frac{1}{2} ln(2\pi) - \frac{1}{2} ln(\sigma_{hm}^2) - \frac{(\varepsilon_t^{hm})^2}{2\sigma_{hm}^2}$$

$$g_p^*(\mu_t, \varepsilon_t^p, \varepsilon_t^{hp} | h_t) = -\frac{3}{2} ln(2\pi) - \frac{1}{2} ln \begin{vmatrix} \sigma_{\mu\mathcal{P}}^2 & 0 & 0 \\ 0 & \sigma_p^2 & 0 \\ 0 & 0 & \sigma_{hp}^2 \end{vmatrix}$$

$$- \frac{1}{2} [\mu_t, \varepsilon_t^p, \varepsilon_t^{hp}] \begin{bmatrix} \sigma_{\mu\mathcal{P}}^2 & 0 & 0 \\ 0 & \sigma_p^2 & 0 \\ 0 & 0 & \sigma_{hp}^2 \end{bmatrix}^{-1} \begin{bmatrix} \mu_t \\ \varepsilon_t^p \\ \varepsilon_t^{hp} \end{bmatrix}$$

$$= -\frac{1}{2} ln(2\pi) - \frac{1}{2} ln(\sigma_{\mu\mathcal{P}}^2) - \frac{\mu_t^2}{2\sigma_{\mu\mathcal{P}}^2} - \frac{1}{2} ln(2\pi)$$

$$- \frac{1}{2} ln(\sigma_p^2) - \frac{(\varepsilon_t^p)^2}{2\sigma_p^2} - \frac{1}{2} ln(2\pi) - \frac{1}{2} ln(\sigma_{hp}^2) - \frac{(\varepsilon_t^{hp})^2}{2\sigma_{hp}^2}.$$

The error terms in this model have a recursive structure. The growth errors are temporally recursive:

$$\mu_t = g_t - \gamma_0 - \gamma_1 pe_t - \gamma_2 mm_t - \rho \mu_{t-1}.$$

Here we assume that $\mu_{1914} = 0$, enabling us to construct the sequence of μ_t's conditional on γ_0, γ_1, γ_2, the sequence of mm_t values, and the sequence of realizations of pe_t. While r_t and pe_t depend on the latest realizations of V_s^p, $s < t$, they are predetermined as of t. Given the contemporaneous growth error, we can calculate the midterm House voting error and the on-year presidential election error, namely,

$$\varepsilon_t^{hm} = v_t^{hm} - \kappa_0 - \kappa_1 r_t - \kappa_2 v_{t-2}^{hp}$$

$$- \kappa_3 mm_t - \kappa_4 g_t - (\kappa_5 - \kappa_4)\mu_t$$

$$\varepsilon_t^p = v_t^p - \psi_0 - \psi_1 r_t - \psi_2 v_{t-2}^{hm} - \psi_3 mm_t - \psi_4 g_t$$

$$- (\psi_5 - \psi_4)\mu_t;$$

and given the on-year presidential voting error, we can calculate the on-year House voting error, namely,

$$\varepsilon_t^{hp} = v_t^{hp} - \lambda_0 - \lambda_1 r_t - \lambda_2 v_{t-2}^{hm} - \lambda_3 mm_t$$

$$- \lambda_4 g_t - (\lambda_5 - \lambda_4)\mu_t - \lambda_6 \varepsilon_t^p.$$

We adopt the following notation:

$\gamma = (\gamma_0, \gamma_1, \gamma_2, \rho, \sigma_{\mu\mathcal{N}}, \sigma_{\mu\mathcal{M}}, \sigma_{\mu\mathcal{P}})$
$\psi = (\psi_0, \psi_1, \psi_2, \psi_3, \psi_4, \psi_5, \sigma_{\varphi p})$
$\lambda = (\lambda_0, \lambda_1, \lambda_2, \lambda_3, \lambda_4, \lambda_5, \lambda_6, \sigma_{hp})$
$\kappa = (\kappa_0, \kappa_1, \kappa_2, \kappa_3, \kappa_4, \kappa_5, \sigma_{hm})$.

We can rewrite the likelihood function as

$$\ell(\gamma, \psi, \lambda, \kappa) = \ell^g(\gamma) + \ell^p(\psi | \gamma, \mu_t)$$

$$+ \ell^{hp}(\lambda | \psi, \gamma, \mu_t, \varepsilon_t^p) + \ell^{hm}(\kappa | \gamma, \mu_t),$$

where

$$\ell^g(\gamma) = -\frac{T}{2} ln(2\pi) - \frac{N}{2} ln(\sigma_{\mu\mathcal{N}}^2) - \frac{M}{2} ln(\sigma_{\mu\mathcal{M}}^2)$$

$$- \frac{P}{2} ln(\sigma_{\mu\mathcal{P}}^2) - \sum_{t \in \mathcal{N}} \frac{\mu_t^2}{2\sigma_{\mu\mathcal{M}}^2} - \sum_{t \in \mathcal{M}} \frac{\mu_t^2}{2\sigma_{\mu\mathcal{M}}^2} - \sum_{t \in \mathcal{P}} \frac{\mu_t^2}{2\sigma_{\mu\mathcal{P}}^2}$$

$$\ell^p(\psi | \gamma, \mu_t) = -\frac{P}{2} ln(2\pi) - \frac{P}{2} ln(\sigma_p^2) - \sum_{t \in \mathcal{P}} \frac{(\varepsilon_t^p)^2}{2\sigma_p^2}$$

$$\ell^{hm}(\kappa | \gamma, \mu_t) = -\frac{M}{2} ln(2\pi) - \frac{M}{2} ln(\sigma_{hm}^2) - \sum_{t \in \mathcal{M}} \frac{(\varepsilon_t^{hm})^2}{2\sigma_{hm}^2}$$

$$\ell^p(\psi | \gamma, \mu_t, \varepsilon_t^p) = -\frac{P}{2} ln(2\pi) - \frac{P}{2} ln(\sigma_{hp}^2) - \sum_{t \in \mathcal{P}} \frac{(\varepsilon_t^{hp})^2}{2\sigma_{hp}^2}.$$

Parameter estimates are obtained via maximum likelihood. We obtain starting values for the coefficients of directly observed variables by first estimating each equation by ordinary least squares. Starting values for the coefficients of the shock terms and for the variances of the shocks are recovered from the covariance structure of the ordinary least squares errors.

Testing the restrictions embodied in equations 28, 30, and 32 could in principle be accomplished by a likelihood ratio test. This would require reestimating the entire model by maximum likelihood, a nontrivial task. We instead adopt Rothemberg's (1973) method, a more manageable, asymptotically equivalent procedure.

Notes

This paper was initiated while Alesina was an Olin Fellow at the National Bureau of Economic Research (NBER), Londregan a faculty member at Carnegie Mellon, and Rosenthal a visiting professor of economics at the Massachusetts Institute of Technology. Alesina thanks the Olin and Sloan foundations for financial support. Alesina and Rosenthal acknowledge financial support from National Science Foundation Grant No. SES-8821441. The paper was revised while Rosenthal was a fellow at the Center for Advanced Study in the Behavioral Sciences (CASBS). He is grateful for financial support provided by National Science Foundation Grant No. BNS-870864 during his stay at CASBS. Very useful comments were provided by Gregory Hess, Susanne Lohmann, and our university colleagues; participants in seminars at Houston, the Institute of International Economics in Stockholm, NBER, Princeton, Rochester, and Stanford; participants in panels at the 1991 meetings of the Midwest Political Science Association and American Political Science Association; and participants in conferences sponsored by Washington University in St. Louis in 1991 and the European Science Foundation in Sesimbra, Portugal, in 1992.

1. The seminal work in this area is by Hibbs (1977, 1987). See also Alesina 1988; Alesina and Roubini 1992; Alesina and Sachs 1988; Chappell and Keech 1988. Beck (1982) suggests that in addition to partisan effects, there are important administration specific effects, as well.

2. But Kiewiet and Udell (1991) find that Kramer's (1971) initial results change when recently improved historical data series are used in place of his original data.

3. See Jacobson 1990 for a contrasting view.

4. Similar partisan effects are observed in many other industrial democracies; see Alesina and Roubini 1992 and the references cited therein.

5. Most wage contracts last one to three years (Taylor 1980).

6. Our formulation of the competence model closely follows Persson and Tabellini 1990. This model, applied to a different economic problem, was originally proposed by Rogoff and Sibert (1988). Related results are in Cukierman and Meltzer 1986.

7. Allowing for persistence in output growth in equation 1 would not change our qualitative conclusions, given the functional forms used in the model.

8. Results would go through even if utility were also quadratic in growth. See Alesina and Sachs 1988.

9. The model can easily be generalized, without changes in the results, to one in which the government controls the money supply, rather than inflation. See Alesina 1988.

10. This is a very rough characterization of the American electoral system. We ignore the electoral college, the bicameral legislature based on geographic constituencies, and the presence of staggered terms in the Senate.

11. The qualitative results of the model would be preserved if there were serial dependence in **a**.

12. It is natural to think of the inflation rate, π, as a percentage. In this context, it may seem strained to think of the desired rate as having a range of only 1% and, for $\mathbf{a} < 0$, to include desired deflation. Equations 5 and 6, however, are arbitrary scaling used to simplify the algebra. The analysis would not be changed by allowing for a wider uniform support limited to positive ideal points.

13. Equation 7 uses standard results in signal extraction theory presented in Cukierman 1984.

14. To guarantee that the pivotal voter's own ideal point is uninformative about the realization of the random variable **a**, we further assume that $0 < w < \min\{\bar{\pi}^{R^*} - \gamma b, 1 - \bar{\pi}^{D^*} + \gamma b\}$. This guarantees that the fraction of the electorate without weakly dominant strategies of voting is uniformly distributed over the interval $(\bar{\pi}^{D^*}, \bar{\pi}^{R^*})$ for *any* realization of **a**.

15. See Alesina and Rosenthal 1991 for technical derivations. Since we have a continuum of voters, the coalition-proof Nash concept is implemented by using recent results of Greenberg 1989.

16. Note that voter expectations depend upon the election forecasts, represented by Q, of the agents in the economy. Say that the agents in the economy had some forecast $\hat{Q}$. It might be thought that $\bar{\pi}^P$ depended on $\hat{Q}$, so we would require $\bar{\pi}^P(\hat{Q})$; and at the wage-setting stage, wages would be set taking into account the "reaction function" of the electorate. However, the functional forms in our model imply that $\bar{\pi}^P$ is independent of the electoral forecasts of the agents in the economy. The basic intuition is that increasing $\hat{Q}$ makes for a larger recession in the case of an R victory but at the same time results in a smaller expansion if D wins. The *difference* in growth rates offered by the parties remains constant as $\hat{Q}$ varies. Thus, there is a single value of $\bar{\pi}^P$.

17. Mankiw, Miron, and Weil (1987) provide empirical evidence of an important regime shift in 1914.

18. For a recent discussion and survey, see Romer and Romer 1989.

19. We did experiment with a specification that allowed for a constant Q different from 1/2. This entailed estimating separate impacts for Democrats and Republicans. We were not able to reject the null hypothesis of equal coefficients (Q = 1/2), a not-surprising result given our small sample size.

20. The price of oil represents an additional source of transitory shocks. As adding a measure of oil prices to our growth equation leaves results (available on request) virtually unchanged and uses degrees of freedom, we have chosen not to use this variable.

21. Note however that the GNP growth series can be well described by either an MA(1) or an AR(1) process. Our choice of the MA(1) representation was driven by the tractability of the theoretical model in this case. See Christiano and Eichenbaum 1989 and Campbell and Mankiw 1987 on the difficulty of discriminating among ARMA models of GNP growth.

22. This model, in which growth is MA(1), implies that transitory shocks to growth have permanent effects on the level of output. The alternative hypothesis that the economy reverts to its long-term trend level is tested and rejected by Campbell and Mankiw (1987), in favor of the hypothesis that growth shocks have permanent effects. See also Christiano and Eichenbaum 1989.

23. Note that we assume that the impact of competence is immediate, whereas the inflationary surprise occurs only in the second year of each administration. If there were a similar gestation lag between the implementation of policies related to the competency dimension of the executive and their effects, retrospection would be of no use in assisting rational voters' inferences about an incumbent candidate's postelection effectiveness. Moreover, we found that the standard MA(1) model could not be rejected in favor of a model of growth with lagged competency. Results are available on request.

24. Under H_1, the likelihood ratio test statistic of .402 is drawn from a χ^2_1 distribution, yielding a p-value of .53.

25. We also make the simplifying assumption that $\mu_{1914} = 0$.

26. Many analysts (see Erikson 1989) include a direct measure of the incumbent president's popularity on "noneconomic" dimensions. This is typically constructed from opinion poll data collected a few months prior to the election or immediately after the election. In our model "personality" effects are incorporated in the error term φ^P_t, while the lagged

House vote tracks evolving differences between the parties' ideal points and those of the voters. While measures of individual candidate effects would be desirable, the standard measures are subject, as Fair (1978) pointed out, to simultaneity bias (leaving aside questions of data availability prior to 1948). Consequently, we do not include survey-based measures in our specification.

27. If, following our formal model, there were competency-based voting in presidential, but not congressional, elections, it would be possible to have a negative coattails effect, also. The probability of reelecting the incumbent would increase with greater competence, reducing, via equation 24, the vote for the incumbent's congressional party.

28. It is important to control for demobilization, as well as mobilization, in evaluating the partisan surprise to the economy. In particular, the massive demobilization following World War II picks up the recession of 1946, the second year of a Democratic watch.

29. This estimate is within one standard deviation of Fair's (1988) estimate that an extra 1% of growth corresponds to an additional 1.01% of the incumbent growth share.

30. See Popkin 1991, pp. 91–92, for discussion of this point.

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Alberto Alesina is Paul Sack Associate Professor of Political Economy, Harvard University, Cambridge, MA 02138.

John Londregan is Assistant Professor of Politics and Public Affairs, Princeton University, Princeton, NJ 08544-1012.

Howard Rosenthal is Professor of Industrial Administration and Political Science, Carnegie Mellon University, Pittsburgh, PA 15213.

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GOVERNMENT FORMATION AND ENDOGENOUS PARTIES

DAVID P. BARON *Stanford University*

I provide a formal theory of government for a political system characterized by a proportional representation electoral system, a parliamentary government that exercises collective responsibility, and a government formation process. Political parties are assumed to be policy-oriented and to serve the interests of those who vote for them. Parties choose policy platforms that determine their representation in parliament; and given that representation, the parties bargain over the government to be formed and the policies that government will implement. The model yields equilibria with the property that parties choose dispersed policy positions. Thus, electoral incentives in proportional representation parliamentary systems need not lead to policy convergence. The theory provides predictions of party locations such as those developed in the manifesto project.

Rae's (1971) pioneering study of electoral systems focused on the proportionality of representation in legislatures and the number of parties, using as explanatory variables the electoral system, the ballot structure, and the districting system. Lijphart (1990) improved on and extended Rae's analysis, and Taagepera and Shugart (1989) also examined the relationship between the structure of electoral systems and representation. Representation, however, is an endogenous function of both institutional structure (such as that studied by Rae, Lijphart, Taagepera, Shugart, and others) and the strategies adopted by parties and voters in the context of that institutional structure. These studies tend to focus on representation, rather than on the policies that parties advocate in elections and take into the government formation process. Yet policies are a principal instrument of party competition and a focus of government formation. I shall provide a theory of parliamentary government and electoral competition with policy-oriented parties in which the policies of parties, the electoral support those policies generate, the government formed, and the policy implemented by that government are endogenous.

In multiparty systems in which no single party has a majority, a natural focus is on coalition governments. Most studies of coalition governments in parliamentary systems (de Swaan 1973; Dodd 1976; Strom 1990) focus on the government formed as identified by the parties holding portfolios. Some studies resulting from the manifesto project are based on data on policy positions of parties and on the relation between those positions and the governments formed (Budge and Keman 1990; Budge, Robertson, and Hearl 1987; Laver and Budge 1992; Laver and Schofield 1990). These studies generally find that the policies of parties are dispersed in the policy space, rather than convergent. These studies also tend to focus on locating the positions of parties, rather than as viewing them as resulting from a strategy for electoral competition directed at the subsequent government formation stage.

The theory guiding these studies is based on cooperative game theory (Axelrod 1970; Riker 1962) and focuses on properties of coalitions with the core or its variants as the solution concepts.[1] Other theories have been developed using solution concepts related to the core (Aumann and Maschler 1964; McKelvey, Ordeshook, and Weiner 1978; and Wilson 1971). The properties of these solutions (e.g., minimal winning coalitions and connected coalitions) have been criticized because of their apparent inconsistency with the data indicating that a substantial number of governments are minority ones, others include surplus parties (Budge and Keman 1990; Strom 1990), and others are not connected.

The renewed interest in explaining governments in terms of the policy positions of parties has produced extensive data sets and analyses that have outstripped the predictive capabilities of theories based on the core. This has led some researchers to develop behavioral theories that depend on both institutional and preference-based factors (Budge and Keman 1990; Budge, Laver, and Strom 1991). I share those objectives but, instead of a behavioral theory, provide a formal theory incorporating both institutions and policy-oriented parties and voters. Equilibria result in this setting because of the sequential structure of the political process and the use of a noncooperative equilibrium concept.

Austen-Smith and Banks (1988) and I (Baron 1991) have shown that the government formation process gives rise to equilibrium governments and that the characteristics of that process have important effects on the government formation strategies of parties and on the policy implemented by the government formed. By incorporating the government formation process as an element of the institutional structure, Austen-Smith and Banks are able to study the relationship between the government formation process and the electoral representation of parties.[2] The theory presented here is in the same spirit and investigates the endogeneity of party policy positions, their electoral support, the government formed, and its policies. The institutional structure includes a parliamentary system, a proportional representation electoral system, and a sequential government formation process.

The nature of the parliamentary government is also important to the choice of policies. Austen-Smith and Banks (1990) and Laver and Shepsle (1990) present theories of government formation based on the notion that governments delegate policy authority to ministries. The restrictions this delegation imposes on the possible coalitions can yield a nonempty core.

The type of parliamentary government considered here is one in which the cabinet exercises collective responsibility for the policy implemented by the government formed. Although the allocation of ministries may be important in the implementation and administration of policies, those policies are considered here to have been agreed on during the bargaining over government formation. Such agreements are credible because breaking the agreements results in the failure of the government and the possibility that a party in the government may be excluded from the next government formed. This approach does not rule out any possible governments nor does it restrict the policies that governments can implement. Coalition governments thus always form around policies in their Pareto set. In contrast, Austen-Smith and Banks (1990) and Laver and Shepsle (1990) restrict the set of feasible policies, and the resulting policies of coalition governments are generally not in the Pareto set of the government parties.[3]

The choice of policy positions by parties depends importantly on the dimensionality of the policy space. In one dimension, the median voter theorem identifies equilibrium positions for two-party elections. Equilibria can also result with more than two candidates (Cox 1987, 1990a, 1990b). In higher-dimensional policy spaces, however, equilibrium locations are unlikely to exist unless restrictions are placed on feasible locations or additional strategic considerations, such as entry deterrence of a new party, are considered.[4] In the model considered here, the policy space is multidimensional and the electoral system is proportional representation, so that equilibria do not exist in the absence of additional institutional structure. The institutional structure that gives rise to equilibrium locations of parties is the government formation process.

In modeling government formation, party policy choices, and the electoral support of parties, a number of assumptions must be made about the preferences and the sophistication of the actors. The actors are naturally voters and parties. It seems reasonable to model parties as being sophisticated about their choices of policy positions in light of their understanding of the electoral system and the government formation process. It is less reasonable to assume that voters are as sophisticated as parties. Consequently, parties will be represented as strategic players; and voters will be assumed to be limitedly rational.

Parties are assumed to understand the government formation process and the interactions between their choice of policy positions and the strategies of the other parties. Parties also understand the voter support they will receive as a function of their choice of policy position. That position is to be understood as the party's platform or manifesto. A platform serves two purposes. First, it is the basis on which voters choose which party to support in the election. Second, subsequent to the election, it is the position from which the party bargains over the government to be formed and the policy to be implemented by that government. The policy implemented by the government need not be—and in general is not—the same as the platform of any of the parties; that is, in bargaining over government formation, parties make concessions to their government partners.

Voters are assumed to be only limitedly sophisticated and unable to conduct a full equilibrium analysis of the government formation process. More specifically, voters are assumed to know the platforms of the parties and to understand that a multiparty government will result from bargaining in the government formation process. Voters, however, are not sufficiently sophisticated that they can make predictions about which government will form as a function of how they cast their vote. Instead, voters are assumed to believe that the party with a platform closest to their ideal point will best represent their interests in the bargaining during the government formation process.

One justification for this assumption is that the parties are assumed to be policy-oriented and to serve the interests of their supporters and not to pursue some other interests, such as winning seats or holding ministries in the government. Thus, parties have derived policy preferences reflecting the preferences of their supporters; parties are not concerned about the number of votes received other than through its effect on the policies implemented by the government formed; and representation is instrumental to policy-oriented parties that seek to serve the interests of their supporters. Since voters support the party whose platform best represents their interests, a party's representation is consistent with how well it serves voters' interests. Thus, parties are instruments of their supporters and agents for their interests in the government formation process;[5] and the set of supporters is endogenous to the choice of a party's platform as induced by the government formation process.

More generally, I shall study a political system characterized by its (1) electoral system, (2) legislative system, and (3) government formation process. The legislative system is assumed to be parliamentary, and the electoral system is pure proportional representation with no election threshhold. Two government formation processes will be considered here. The first is probabilistic, with the probability that a party will be accorded the opportunity to form a government equal to the proportion of seats it holds in the parliament. The second involves a fixed order (determined, e.g., by their representation), by which the parties are selected to attempt to form a government.[6]

To facilitate the analysis, the number of parties is assumed to be fixed at three. The number of parties, thus, is not endogenous; but the set of voters that support each party and the platform policies of the parties are endogenous. These policies correspond to those which are the focus of the manifesto project (Budge, Robertson, and Hearl 1987). The outcome of the bargaining is a government formed and the policy that that government will implement. That policy corresponds to those which are the subject of the study reported by Laver and Budge (1992).

The objectives of the following analysis are to establish and to characterize equilibrium party configurations and policy positions. Portions of the analysis represent preequilibrium analysis in which incentives are investigated under the assumption that the policy positions of two of the parties are fixed—as by their past, for example. In the model, the choice of policy position reflects two incentives. On the one hand, the party has an incentive to choose a policy close to those preferred by its own supporters. On the other hand, the party has an incentive to choose a policy close to those of the other parties to reduce the variability in the policies of the possible governments that might be formed subsequent to the election.

When all the parties choose policies, equilibria can exist as induced by the government formation process. If the probability that a party will be selected to form a government is proportional to its representation in the parliament, two equilibria are identified. In one, the equilibrium policies are separated; and in the other, all three parties locate at the center of the policy space. The theory thus predicts that party positions can be, but are not necessarily, dispersed in the policy space. When the government formation process involves a fixed order of selection based, for example, on the rank size of party representation, policies at the center of the policy space are not an equilibrium. An equilibrium exists, however, with the parties choosing policies separated and away from the center of the set of voter preferences. Party positions are thus dispersed. The theory, thus, is capable of providing an explanation for the dispersed policy locations identified in the manifesto project.

I shall introduce the model and a government formation process in which the probability of selection to form a government is equal to its proportion of seats in the parliament and characterize the political equilibrium for this government formation process. Then, I shall consider a different government formation process in which parties are selected to form a government in the order of their representation. Finally, I shall offer some discussion of the results and the perspective taken here.

THE MODEL

Actors and Institutions

The model represents a simplified version of a political system in which no single party can be expected to have a majority and hence one in which a multiparty government will form. The political system is characterized by its institutional structure, comprised of a proportional representation electoral system, a government formation process in which parties bargain over both the formation of a government and the policies the government will implement, and a parliamentary system in which a majority of the members must approve investiture of a government or defeat a motion of no confidence in that government. A government formed is thus identified by two characteristics: a policy to be implemented and the parties that support that policy on the investiture vote.

The actors in the political system are parties and voters. Prior to the election, parties choose platforms, which consist of a policy position that will be used in the bargaining over government formation. The electoral system is a pure party-list system in which voters have one vote and cannot abstain. Voters choose to support parties based on their platform policies.

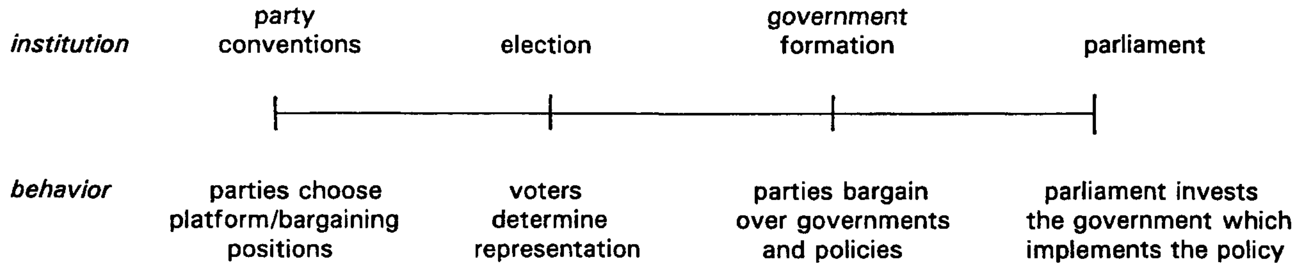
A political party is represented as a unitary actor and has the objective of serving the interests of its supporters by representing them in the bargaining over policies in the government formation phase of the political process.[7] In the tradition of the spatial theory of electoral competition, parties are assumed to choose their platform positions from a multidimensional policy space.[8] A party chooses its platform strategically based on the likely consequences for its supporters in the government formation stage. Parties thus are agents of their supporters.

The sequence of actions in the political system is indicated in Figure 1. Prior to the election, a party chooses its platform in a party convention according to the aggregate preferences of the voters who are anticipated to support it in the election. That platform is the bargaining position the party uses in the subsequent bargaining over government formation. In the election, voters vote based on those platforms, and the election determines the representation of each party in the parliament. Based on that representation, the parties bargain, as structured by the government formation process, over the policies to be implemented by the government. That government exercises collective responsibility for its policies, and the equilibrium policies are those characterized in my earlier work (Baron 1991). A party is assumed to be constrained in the government formation process by its platform, which provides the basis for the expectations on which voters choose among parties.[9] The party is thus constrained by its supporters to bargain from its platform position. If the party were subsequently to disavow its platform, voters could be thought of as penalizing the party in future elections.

An alternative interpretation of a party platform is that the party selects a leader who has considerable latitude in bargaining with other party leaders and thus in the government formation process can serve personal policy preferences. The party cannot control its leader once chosen but can choose its leader based on its knowledge of the leader's policy preferences. Once chosen, the leader engages in bargaining over

FIGURE 1

Time Sequence in the Political System


institution
party conventions
election
government formation
parliament
behavior
parties choose platform/bargaining positions
voters determine representation
parties bargain over governments and policies
parliament invests the government which implements the policy

government formation and may become the prime minister. In this interpretation, the preferences of the party leader constitute the bargaining position the party takes into the government formation process and represents the party platform in the election. The choice of a platform or of a leader will be referred to as the party's policy position.

Policies and Voter Preferences

The policies implemented in the parliament by the government formed are represented by a vector $w = (w_1, \ldots, w_n) \in W \subset \Re^n$, $n \geq 2$. Voters have preferences represented by a utility function $u(w; s)$, where $s \in S$ is a parameter of preferences that differs among voters. The set of voters is described by a distribution function $F(s)$ on S, the set of voters' preference parameters. To simplify the analysis, voters are assumed to have Euclidean preferences, where $s \in \Re^n$ is a voter's ideal point. A voter with ideal point s thus has a utility function given by

$$u(w; s) = -\sum_{i=1}^{n} (w_i - s_i)^2. \qquad \textbf{(1)}$$

In the specific case considered, the set S is taken to be a disk of radius 1 with center at the origin and contained in W. The ideal points are assumed to be uniformly distributed on S, and the total number of voters is normalized to one.

Parties, Preferences, and Policy Positions

Each of the m parties chooses a policy position $b_j \in S \subset W$, that serves both as its electoral platform and its bargaining position in the subsequent bargaining over the formation of a government. Denoting the set of voters supporting party j by S_j, the aggregate utility $v_j(w; S_j)$ of the supporters of party j when policy w is implemented by the government formed is

$$v_j(w; S_j) \equiv \int_{S_j} u(w; s) dF(s). \qquad \textbf{(2)}$$

The function v_j is thus a social welfare function that party j uses in serving the interests of its supporters. That objective is represented as the average utility of its members; and to simplify the analysis, the parties are assumed not to discount the future.[10] The average utility $Av_j(w; S_j)$ is

$$Av_j(w; S_j) \equiv \frac{v_j(w; S_j)}{|S_j|},$$

where $|S_j|$ denotes the proportion of supporters of party j. Note that a party may prefer to be small if by so doing it can make its members better off. The party may, however, have an incentive to become larger if that makes it more likely to be in the government and hence more likely to be able to implement policies that serve the interests of its supporters.

A party chooses its policy position b_j in anticipation of its electoral consequences and the subsequent government formation equilibrium. At the time that the parties choose their policy positions, the policy w that will be implemented by the government formed may be uncertain, as a consequence of the features of the government formation process. Thus, the policy positions and the policies implemented are not necessarily—and in equilibrium will not be—the same. Let $G(w; \bar{b}, \bar{S})$ denote the distribution function of the equilibrium policies as induced by the policy positions $\bar{b} = (b_1, b_2, \ldots, b_m)$ of the parties and by their sets of supporters $\bar{S} = (S_1, S_2, \ldots, S_m)$. The distribution is determined from the government formation equilibrium characterized by Baron (1991). The expected aggregate utility $U_j(\bar{b}, \bar{S})$ for a party that chooses a policy position b_j is

$$U_j(\bar{b}, \bar{S}) \equiv \int_W v_j(w; S_j) dG(w; \bar{b}, \bar{S}).$$

The party is assumed to choose b_j to maximize the expected average utility AV_j of its supporters, or

$$b_j \in \underset{b \in W}{\arg\max}\, AV_j \equiv \frac{U_j(b_1, \ldots, b_{j-1}, b, b_{j+1}, \ldots, b_m, \bar{S})}{|S_j|}$$

$$= \int_W AV_j(w; S_j) dG(w; \bar{b}, \bar{S}).$$

Voters are not as sophisticated as parties and vote for the party with a platform or policy position closest to their own ideal point. Voters, thus, trust that the party has chosen its position to serve the interests of its supporters; and that trust is fulfilled in an equilibrium. Since there are an infinite number of voters, an individual voter cannot affect the representation parties have in the parliament. In that case, voters are assumed to receive consumptive value from voting for the party that best represents their interests.[11] If voters believe that their votes will affect the representation of parties, they will vote for the party whose policy position is closest to their ideal point. Voter s thus votes for party j if s prefers j's policy position to the policy position of any other party, or

$$u(b_j;\, s) \geq u(b_k;\, s),\ \forall\, k \neq j.$$

The supporters S_j of party j are thus

$$S_j \equiv \{s \mid u(b_j;\, s) \geq u(b_k;\, s),\ \forall\, k \neq j\}.$$

The vote share p_j, or representation, of party j is thus

$$p_j = \frac{\mid S_j \mid}{\mid S \mid}.$$

A political equilibrium can now be defined.

Definition. *A political equilibrium is a configuration of policy positions* $(b_1^*, b_2^*, \ldots, b_m^*)$*, votes, and government formation strategies satisfying*

1. *that voter s votes for party j if and only if*

$$u(b_j^*;\, s) \geq u(b_k^*;\, s),\ \forall k \neq j,$$

and randomizes among the parties if he is indifferent;

2. *that party j, j = 1, . . . , m, chooses b_j according to*

$$b_j^* \in \arg\max_b \frac{U_j(b_1^*, \ldots, b_{j-1}^*, b, b_{j+1}^*, \ldots, b_m^*, \bar{S})}{\mid S_j \mid};$$

and

3. *a government formation equilibrium that results in a distribution $G^*(w^*;\, \bar{b}^*, \bar{S}^*)$. That equilibrium is a stationary, subgame perfect Nash equilibrium which consists of (a) proposals w^* made by a party when accorded the opportunity to form a government and (b) rules specifying which policies a party will vote for or against at the time of investiture.*

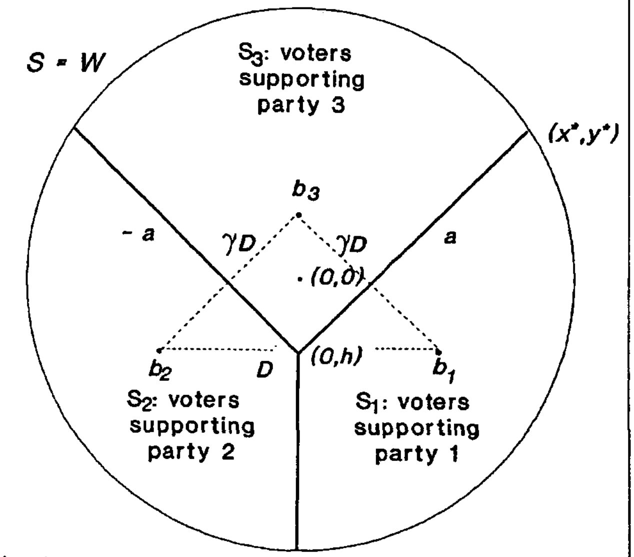
A political equilibrium thus consists of (1) sets of voters each of whom supports the party with a policy position closest to his or her ideal point, (2) a vector of party policy positions each of which maximizes the average expected utility of the party's supporters and from which the parties bargain in the context of a government formation process, and (3) a government formation equilibrium in which the government and its policies are determined.

A Two-Dimensional Specification

A two-dimensional specification with three parties will be used in the following analysis. A basic configuration of party identifications of voters is illustrated in Figure 2 for the case in which S is a disk of radius 1. The location of the positions of parties 1 and 2 (b_1 and b_2, respectively) is symmetric about the center; and b_3 is equally distant from those positions. Voters are identified with parties as indicated by the sets in the figure. For $F(s)$ uniform, the number $\mid S_j \mid$ of voters is the area of those sets. The aggregate preferences of those voters are represented by the function $v_j(w;\, S_j)$, which is quadratic with ideal point t_j given in the Appendix. The Appendix presents an analytical representation of the sets S_j and the functions $v_j(w;\, S_j)$.

FIGURE 2

Party Configurations


S = W
S_3: voters supporting party 3
(x^*,y^*)
b_3
− a
γD
γD
a
$\cdot\,(0,0)$
b_2
D
(0,h)
b_1
S_2: voters supporting party 2
S_1: voters supporting party 1

b_j: bargaining position
γ,D: location parameters
a,h,(x^*,y^*): party voters parameters (Appendix)

The policy position $b_j \in W$ of party j is one component of a party's strategy. The connection between the policy w implemented and the policy positions $(b_1, \ldots, b_M)$ is determined by the government formation process. To facilitate the analysis, only configurations of policy positions reflecting the symmetry of the voter and policy space are considered. As in Figure 2, the policy positions of parties 1 and 2 are assumed to be symmetrically located relative to the center of S and are specified as

$$b_1 = \left(\frac{1}{2}D,\, -\beta D\right),\ b_2 = \left(-\frac{1}{2}D,\, -\beta D\right),$$

where β is a constant and D, $D > 0$, is the distance between the policy positions of the two parties. The policy position b_3 of party 3 can then be parameterized in terms of a single choice variable γ, where

$$b_3 = \left(0,\, D\sqrt{\gamma^2 - \frac{1}{4}} - \beta D\right).$$

The policy position b_3 is a distance γD from the policy positions of the other two parties. If $\beta = 1/2\sqrt{3}$, $\gamma = 1$, and $D = 3/\pi$, the policy positions of the parties are the ideal points (or centroids) of the aggregate utility $v_j(w;\ S_j)$ of their supporters. These positions, however, are not the equilibrium policy positions.

Government Formation with Proportional Selection

Because the induced preferences $v_j(w;\ S_j)$ of the parties are quadratic, the theory presented by me elsewhere (Baron 1991) can be used to predict the outcome of the bargaining in the government formation stage. In the first government formation process considered, a party is selected to attempt to form a government with a probability equal to its representation p_i in the parliament. The party selected makes a proposal w to one of the other parties to form a government. That proposal is the policy the government formed will implement if it succeeds in an investiture vote. The party to which the proposal is made may accept the policy by joining with the proposing party to ratify investiture. That policy is then implemented. If the party rejects the proposal by voting against investiture, another party is selected, using the same probabilities, to attempt to form a government.[12] The party to which a proposal is made thus compares that proposal with the equilibrium in the subgame commencing upon rejection. If that proposal is at least as attractive as the prospect of the next round in the government formation process, that party will vote for investiture. The government formed then implements that policy.[13] In the stationary, subgame perfect Nash equilibria of this government formation game, the party initially selected to form a government makes a proposal that is accepted immediately by one other party.[14] The government formation process is thus completed on the first round.[15]

Because of the symmetry of the distribution of voter ideal points, it is sufficient to consider the case in which party 3's policy or bargaining position b_3 is a distance γD from the positions of the other two parties and those two positions are equidistant from the center of S, as specified. Party 3 then has a $p_3 = p$ probability of being selected, and each of the other parties has a probability $(1 - p)/2$. The government formation (stationary) equilibrium for this case is of one of two types depending on the magnitude of p.

The first type occurs when the vote share p of party 3 is below a critical value $p^*(\gamma) \equiv \gamma/(3\gamma - 1) \in [1/3, 1]$. The equilibrium policies then are independent of p, but parties 1 and 2 randomize their policy proposals as a function of p. If party 3 is selected, it makes a policy proposal w^{3i} with probability .5 to attract the support of one of the other two parties for investiture. If party 1 (2) is selected to form a government, it makes a policy proposal to form a government with party 2 (1) with probability r given by

$$r = \frac{p\left(2 - \dfrac{1}{\gamma}\right)}{1 - p}$$

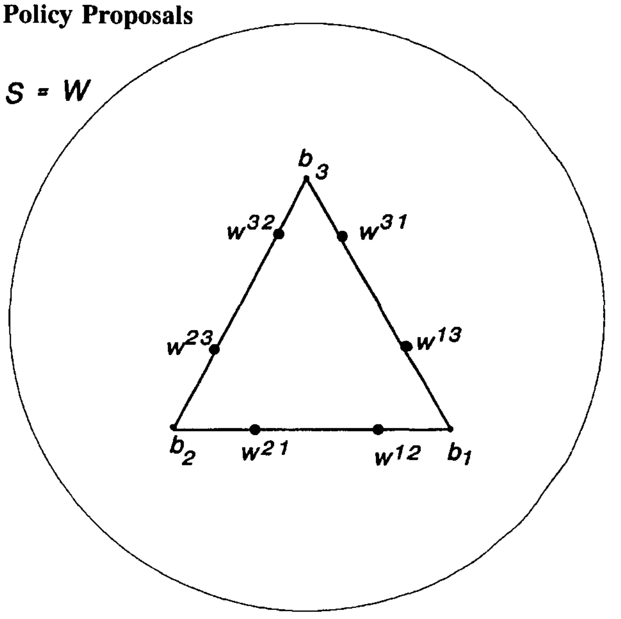
and makes a proposal to party 3 with probability

$$1 - r = \frac{1 - 3p + p/\gamma}{1 - p},$$

where $\gamma = D_{13}/D = D_{23}/D$ and D_{i3} is the distance between the policy positions of parties $i = 1, 2$ and party 3. Since $\gamma \geq 1/2$, then $r \geq 0$, and for $p \leq p^*(\gamma)$, $r \leq 1$. The second type of equilibrium occurs when $p \geq p^*(\gamma)$, in which case $r = 1$. In this case, party 1 always makes a proposal to attract the support of party 2.

FIGURE 3

Policy Proposals



b_i: bargaining position of party i

w^{ji}: policy proposed by party j to party i

Because the government agrees to policies during the government formation process and subsequently exercises collective responsibility for their implementation, the policy proposals w^{ji} made by party j to party i during government formation can be represented as

$$w^{ji}(\lambda_{ji}) = \lambda_{ji}b_j + (1 - \lambda_{ji})b_i, \tag{3}$$

where λ_{ji} is the proportion of the distance along the contract curve from the policy position b_i to the policy position b_j.[16] When the policy positions are equidistant, those policies w^{ji} are located one-third the distance along the contract curve from the position of the proposing party to the position of the other party, as illustrated in Figure 3.[17] As indicated in the figure, the policies implemented by the possible governments are not the policy positions of the parties, since a party must make concessions to its government partner. The policy w^{ji} implemented by a government is thus determined in the government formation equilibrium and influenced both by the policy position of the parties in the government formed and by the policy position of the party not in the government.

For any p, the proposals w^{j3}, $j = 1, 2$, move farther from (closer to) the centroid of the ideal points of the supporters of party 3 whenever the position of party 3 is farther from (closer to) the positions of parties 1 and 2 than they are from each other, that is, whenever $\gamma > 1$. When party 3 is selected to form a government, the proposals w^{3j}, $j = 1, 2$, move closer to the centroid of the ideal points of its supporters. Consequently, party 3's choice of a policy or bargaining position moves some of the government positions closer to, and some farther from, the interests of its supporters. In addition to affecting the government positions, a change in the bargaining position also affects the likelihood p that party 3 will be selected. The choice of a party position thus involves a complex set of incentives.

POLITICAL EQUILIBRIA WITH PROPORTIONAL SELECTION

To determine whether a configuration of policy positions and party supporters as in Figure 2 is a political equilibrium, first consider whether party 3 has an incentive to move its policy position toward the center of S. As already indicated, a movement of the policy position toward the center has two effects: the government formation equilibrium changes, with the policy proposals w^{ji} in equation 3 changing as discussed; and the vote shares of the parties change. As long as the vote shares are such that $p_3 = p \leq p^*(\gamma)$, however, the vote shares do not affect the policy proposals in the bargaining equilibrium. The vote shares, however, affect the probability r that parties 1 and 2 will form a government with each other, rather than party 3.

The expected aggregate utility $U_3(\bar{b}, \bar{S})$ for party 3 is given by

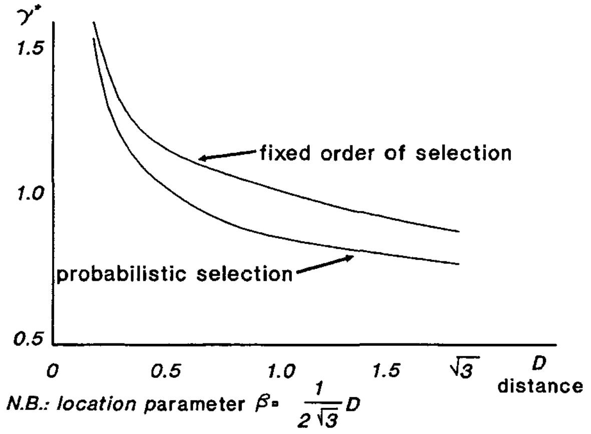
$$U_3(\bar{b}, \bar{S}) = \left(\frac{1-p}{2}\right)(rv_3(w^{12}; S_3) + (1-r)v_3(w^{13}; S_3)) + \left(\frac{1-p}{2}\right)(rv_3(w^{21}; S_3) + (1-r)v_3(w^{23}; S_3)) + p(\tfrac{1}{2}\, v_3(w^{31}; S_3) + \tfrac{1}{2}\, v_3(w^{32}; S_3)). \quad (4)$$

The first term corresponds to the selection of party 1, which makes proposals w^{12} and w^{13} with probabilities r and $1 - r$, respectively. The second term is the corresponding term when party 2 is selected. The third term corresponds to party 3's being selected and making proposals w^{31} and w^{32} with probabilities one-half each. Because of the symmetry of the locations of policy positions, $v_3(w^{12}; S_3) = v_3(w^{21}; S_3)$, $v_3(w^{13}; S_3) = v_3(w^{23}; S_3)$, and $v_3(w^{31}; S_3) = v_3(w^{32}; S_3)$. Then equation 4 simplifies to

$$U_3(\bar{b}, \bar{S}) = (1-p)\,(rv_3(w^{12}; S_3) + (1-r)v_3(w^{13}; S_3)) + pv_3(w^{31}; S_3).$$

FIGURE 4

Optimal Bargaining Positions



The average expected utility AV_3 is then

$$AV_3 = \frac{U_3(\bar{b}, \bar{S})}{|S_3|}. \quad (5)$$

The function AV_3 is too complex to analyze analytically as a function of the location parameter γ, so numerical evaluation will be used. The analysis will first be presented for the case in which parties 1 and 2 have fixed policy positions and party 3 chooses its policy position to maximize AV_3 in equation 5. This may be interpreted as corresponding to the situation in which two parties have policy positions fixed by their past record.

Party 3 has an optimal location γ^*, presented in Figure 4, as a function of the distance D between the policy positions of parties 1 and 2. The optimal location reflects two incentives. First, the preferences in equation 1 exhibit risk aversion; and a party (with its supporters) prefers to have lower dispersion of the policies w^{ji} of the possible governments. This incentive induces party 3 to move toward the policy positions of the other two parties. Second, a party has an incentive to choose a policy position close to the preferences of its supporters because that pulls the policies of some of the possible governments toward the ideal points of its supporters.[18] As indicated in Figure 4, the optimal location parameter γ^* for party 3 is a decreasing function of the distance D between the policy positions of parties 1 and 2. Party 3 thus locates itself relatively farther from the other two parties, the closer their positions are to one another. In doing so, party 3 may accept a lower vote share. Conversely, the farther apart the positions of the other two parties, the closer party 3 prefers to be to them so as to reduce the variation in the possible government positions.

When all three parties choose policy positions, a political equilibrium reflects either a balancing of these two incentives or the dominance of one incentive. There are two equilibria. In one, the parties choose different positions, so that the political system can result in a dispersion of party positions as the parties strive to serve the interests of their supporters. The political system also has an equilibrium in which all three parties choose positions at the center of the space. In this equilibrium, the parties are trapped by the majority rule requirement for investiture, since if one party moves from the center, it is unable to obtain a majority of the vote and must form a government with one of the other parties, each of which has an incentive to reject any policy proposal not at the center, in favor of waiting for its own opportunity to form a government.

The following proposition establishes that all three parties choosing policy positions at the center of S is a political equilibrium and is the only equilibrium in which parties choose the same policy position.

PROPOSITION 1. *For a government formation process in which the probability of selection equals the proportion of seats held, all three parties choosing policy positions at the center and one-third of the voters voting for each candidate form a political equilibrium. The center is the only policy position that is an equilibrium with all parties choosing the same policy.*

Proof. To show the second part, suppose that all three parties choose the same policy and that it is not at the center; then a party could move toward the center, capture a majority of votes, and implement its policy position, which would benefit its supporters. Unless the parties are all located at the center, some party has an incentive to do so.

To show the first part, suppose that all three parties are located at the center. If one party (say, party 3) were to move its policy position away from the center, it will have a share p of the vote that is less than one-half. If one of the other two parties is selected to attempt to form a government, it will propose to form a government with the other party located at the center, and the policy implemented will be at the center; that is, $r = 1$ and $w^{ij} = (0, 0)$, $i = 1$, $j = 2$ and $i = 2$, $j = 1$. The expected aggregate utility $U_1(\bar{b}, \bar{S})$ is then

$$U_1(\bar{b}, \bar{S}) = (1 - p)v_1((0, 0); S_1) + p\left(\frac{1}{2}\, v_1(w^{31}; S_1) + \frac{1}{2}\, v_1(w^{32}; S_1)\right),$$

where w^{3i}, $i = 1, 2$, is the proposal that party 3 makes to party 1 when party 3 has the opportunity to form a government. Party 1 will accept w^{31} if and only if $v_1(w^{31}; S_1) \geq V_1$, where V_1 is the continuation value of the subgame commencing if party 1 rejects party 3's proposal.[19] In a stationary equilibrium with all parties located at the center, the parties reject proposals away from the center, so the continuation value is $V_1 = v_1((0, 0); S_1)$. Consequently, any proposal by party 3 closer to its supporters' preferences than $w^{31} = (0, 0)$ will be rejected and cannot be an equilibrium. Q.E.D.

This proposition may be interpreted as a median voter result for multiple parties. However, it is not robust. The analysis just given pertains to the case in which the parties—or, more correctly, their supporters—do not discount the future and hence maximize their average utility per unit of time. If parties do discount the future, the policy in place during the government formation process becomes important. In that case, if a caretaker government implements the status quo q, the political equilibrium depends on the location of the status quo. Rather than attempt to characterize the equilibria as a function of q, I shall discuss the intuition underlying its impact. First, if q is at the center of the space, the result of Proposition 1 continues to hold. The parties remain trapped at the center by majority rule. If the status quo is not at the center, however, all parties locating at the center is not an equilibrium. Second, when the status quo is not at the center, the (stationary) government formation equilibria vary smoothly in the discount factor. Compared to the case in which the parties do not discount the future, however, the equilibrium proposals are always closer to the policy position of the party selected to form a government. The government formation proposals thus have more dispersion, which strengthens the incentive for a party to locate closer to the bargaining positions of the other parties. To separate the incentives for the choice of a bargaining position from the effects of the location of the status quo, my analysis will focus on the case in which the parties do not discount the future.

Because of the symmetry of the model, a political equilibrium in which parties choose different policy positions can exist only if there is a D such that for each party the optimal γ equals 1. Such an equilibrium must involve symmetric locations about the center. Numerical evaluation demonstrates that there is such an equilibrium when $D = .485$; that is, if the three parties are located equally distant from the center and equally distant from each other at a distance of $D = .485$, no party has an incentive to change its policy position. A dispersion of party positions results. A party balances the incentive to move toward the policy positions of the other parties to reduce the dispersion and the incentive to move away from those positions to move the government policy, if it is selected, toward those preferred by its supporters. This is stated as Result 1.

RESULT 1. *For a government formation process with proportional selection, there is an equilibrium in which all three parties choose policy positions that are equidistant from the center and at a distance $D = .485$ from each other. The parties receive one-third of the vote.*

The government formation process can thus induce equilibrium policies and parties. Those locations can be either separated or identical; but because of the symmetry of the model, they will be symmetric in the policy space. The political parties thus receive an equal share of the vote.[20] Even though they receive an equal share of the vote, their bargaining positions do not maximize the number of seats they will receive. As will be indicated in Result 3, the seat-maximizing position is farther from the center than that in Result 1.

As an example of the application of this result, consider the case of the 1974 election in Luxembourg using data developed in the manifesto project (Budge, Robertson, and Hearl 1987). The three largest parties received 18, 17, and 14 seats, and the smallest of these parties was equidistant from the two other parties at a distance parameterized (approximately) by $\gamma = 2/3$.[21] The theory presented here can explain this location and the corresponding representation. If the positions of the two larger parties were fixed and located by $D = .85$ and $\beta = .1$, the optimal location parameter is $\gamma^* = .78$ and the probability $p_3 = .28$. The predictions are thus quite close to the locations developed in the manifesto project for this election. If all three parties were free to choose their positions, the equilibrium would be locations with D approximately .5 and $\gamma^* = 1.0$. Of course, there is no information available on the distribution of voter preferences along the dimensions identified in the manifesto project.

GOVERNMENT FORMATION WITH A FIXED ORDER OF SELECTION

The government formation process just considered is based on proportional selection; but in some countries, there is a requirement or tradition whereby parties are selected in the order of the size of their representation. Furthermore, some countries have large parties from which one is selected to form a government together with small parties that are never selected. For example, in Germany the Free Democrats are never selected yet are always in the government. A natural question in this case is whether the small party prefers to remain small to ensure that it is an attractive government partner for whichever of the two large parties is given the opportunity to form a government.

The first case considered is that in which the government formation process involves two large parties, either of which may be selected to form a government, and one small party, which will never be selected. In the government formation process, one of the large parties is selected; and if it fails to form a government, the other large party is given the opportunity to do so. If that party fails, the first party then is given another opportunity, and they alternate thereafter. For the configuration of policy positions in Figure 2, there is a continuum of stationary subgame perfect Nash equilibria of this government formation process; but in each of them, a large party selected makes a proposal to form a government with the small party. That proposal is such that the small party accepts it, so the small party is always in the government.

For the case in which the policy position of the small party is equidistant from the policy positions of the other two parties (as suggested by the symmetry of the model), the equilibria are characterized by the following parameters λ_{i3} of the proposals in equation 3 made to the small party 3:

$$\lambda_{i3} \geq 1 - \frac{1}{2\gamma}, \quad i = 1, 2. \tag{6}$$

The lower bound in equation 6 is determined by the opportunity to form a government with the other large party. If party 1 were to form a government with party 2, the policy would be at the midpoint of the contract curve, which is a distance $D/2$ from b_1. If party 1 forms a government with party 3, the policy given by the bound in equation 6 is a distance $D/2$ from b_1 for all γ.

To investigate the policy choice of the small party, initially consider the case in which the policy positions of parties 1 and 2 are fixed by their past. The representations of the large parties are then equal, so that each will have an equal chance of being selected. The expected aggregate utility $U_3(\bar{b}, \bar{S})$ for the small party of the government formation game thus is

$$U_3(\bar{b}, \bar{S}) = \frac{1}{2}\, v_3(w^{13}; S_3) + \frac{1}{2}\, v_3(w^{23}; S_3),$$

where w^{i3} is the policy proposal made by party i, ($i = 1, 2$) to party 3; and parties 1 and 2 are selected each with probability 1/2. Because of symmetry, $v_3(w^{13}; S_3)$ and $v_3(w^{23}; S_3)$ are equal, and the expected utility of the supporters of party 3 is

$$\begin{aligned} U_3(\bar{b}, \bar{S}) &= v_3(\lambda_{13}b_1 + (1 - \lambda_{13})b_3; S_3) \\ &= v_3(\lambda_{23}b_2 + (1 - \lambda_{23})b_3; S_3). \end{aligned}$$

Even if the policies of the government formed are unaffected by the policy position of party 3, as they are if λ_{i3} is given by the equality in equation 6, party 3 can have an optimal location. This results because party 3 is able to choose a position that attracts a set of supporters who most prefer that policy position. Figure 4 presents the optimal policy position of party 3 as a function of the distance between the policy positions of the two large parties.[22] As indicated in the figure, the optimal policy position is a decreasing function of the distance between the policy positions of the two large parties. This reflects the incentive to reduce the variability of the policies of the possible governments, the farther apart the positions of the two large parties are.

This analysis indicates that with fixed locations of two parties, a third party has an optimal location distant from those of the other two parties. Furthermore, that party may prefer not to be in the selection process. This is summarized as Result 2.

RESULT 2. *In a government formation process with (1) selection in the order of representation, (2) fixed and symmetric positions of two parties, and (3) a party that will never be in the selection process, that party has an optimal location and will be in the government with whichever of the larger parties is selected.*

In the case of Germany, for example, this theory predicts that the Free Democrats could choose a policy position that is centrist on one dimension and differentiated on the other.[23] The Free Democrats typically receive a small proportion of the seats in the Bundestag; and if the Christian Democrats and the Social Democrats, for example, are located at a distance $D = 1.5$ with $\beta = 0$, the theory predicts that the Free Democrats would choose to locate at a distance $\gamma^* D = 1.86$ from them and would receive 5.1% of the vote.[24] If $D = 1.0$ and $\beta = 0$, the theory predicts that the Free Democrats would choose a position at a distance $\gamma^* D = 1.12$ from the other two parties and would receive 15.4% of the seats. Again, data are not available on the distribution of voter preferences.

For some fixed locations of two of the parties, the third party may have an optimal location such that it has the highest vote share. For example, if $D = 1.0$ and $\beta = 1/2\sqrt{3}$, the vote share of party 3 is .38. In that case, all three of the parties may be in the selection process with the order determined by their size. The equilibrium policy proposals w^{ji} then are the midpoints of the contract curves between the policy positions of the party that has the first opportunity to form a government and the party that is next in the order.

A political equilibrium analogous to that in Result 1, with dispersed locations, results. As Proposition 2 indicates, the equilibrium has the property that each party maximizes its vote share, given the policy positions of the other two parties.

PROPOSITION 2. *If all three parties are in the selection process and* $w \in \Re^2$, *a configuration* (b_1^*, b_2^*, b_3^*) *is part of a political equilibrium if and only if* b_i^* *maximizes* p_i, *given the policy positions of the other two parties. The political equilibrium thus has the property that the parties maximize their vote shares.*

Proof. Any equilibrium must be symmetric, since otherwise some party has an incentive to choose a different policy position. The equilibrium policies that constitute a government formation equilibrium are always more favorable to the largest party (which makes the first proposal) than to the smallest party. This results because the first proposal is accepted in equilibrium and is on the contract curve between the largest and the second largest parties. Also, for $\gamma = 1$, the equilibrium policy is always at least as close to the policy position of the largest party than to the policy position of the second-largest party. Suppose an equilibrium is such that the vote shares are equal, yet p_i is not maximal with respect to γ for some party. That party then can change its policy position and obtain a larger vote share than that of either of the other parties. Hence, maximizing p_i for each party is a necessary condition. Sufficiency is immediate, since if one party were to change its policy position, it would become the smallest party, and the equilibrium policy proposals would be farther from its policy position. Q.E.D.

Result 3, obtained by numerical evaluation, characterizes the political equilibrium.

RESULT 3. *If all three parties are in the selection process and a fixed order of selection is chosen randomly when all three parties have the same vote shares, there is a political equilibrium in which all three parties choose policy positions equally distant from the center and at a distance* $D = 1.375$ *from each other. The equilibrium policy positions are farther from the center than are the centroids of the ideal points of the supporters of the party, that is,* $(b_{31} = 0, b_{32} = .794)$ *and* $(t_{31} = 0, t_{32} = .551)$.

The policy positions in this equilibrium are farther from the center than those with proportional selection, because the latter are not vote-maximizing. In the case of three parties and a government selection process in which a party is selected to form a government in the order of their size, the parties in equilibrium actually choose positions farther from the center than are the centroids of their supporters' ideal points. They do so because any other position would allow one of the other parties to obtain a larger vote share and hence be given the opportunity to form a government. This may be interpreted as an anti-median-voter theorem in the sense that electoral incentives drive the positions away from the median. Electoral incentives in proportional representation parliamentary systems, thus, do not necessarily lead to policy convergence.

DISCUSSION

The objective of a positive political theory is to explain the policies implemented by governments and the choices of political actors as they attempt to influence those policies. In a parliamentary system, the principal actors are parties and voters. Their choices are structured by political institutions, including the electoral system, the government formation process, and the parliamentary process through which the policies of the government formed are implemented. Parties choose strategies with attention both to elections and to the subsequent bargaining over government formation, and their choices of platforms may be viewed as balancing electoral incentives and government formation incentives. In the model considered here, parties are instruments of their supporters' interests, and their electoral platforms and policy positions are assumed to be the same. When the voters have policy interests, both platforms and governments are necessarily instrumental.

To explain policies and governments, it is necessary to know the policy dimensions of electoral competition, the distribution of voter preferences, and the institutional structure of the political system. A political equilibrium in a parliamentary system would thus be expected to reflect the characteristics of the government formation process that will ensue after an election has determined the number of seats that parties hold. That process involves bargaining and is modeled here as a noncooperative, sequential bargaining game. A political equilibrium is then comprised of (1) for each party, a policy position maximizing the interests of the voters who support that party in the election and serving as a position from which the party bargains in the government formation stage; (2) for each party, a government formation strategy; and (3) voters voting for the party with the platform they prefer. The outcome of the government formation bargaining is a policy different from the policy position of any party, since a party must make concessions to its government partner.

The manifesto project has developed a spatial representation of party positions; and those positions are dispersed, rather than convergent. The model presented here demonstrates that there are political equilibria in which policy-oriented parties choose dispersed policies. From a comparative perspective, the equilibria depend on the government formation process. In the case in which the probability of selection is proportional to the number of seats a party holds, one political equilibrium has all parties choosing distinct positions located symmetrically about the center of the space of voter ideal points. This equilibrium balances the incentive to reduce the dispersion in the possible government policies by locating close to the bargaining positions of the other two parties with the incentive to locate farther away to pull the policies of the government formed closer to the ideal points of the party's supporters.

In a government formation process characterized by a fixed order of selection, a party may have an incentive to choose a policy position that gives it the smallest vote share if that will make it an attractive coalition partner for the larger parties. This will be the case when the policy positions of two of the parties are fixed and the government formation process has a fixed order in which the small party will never be given the opportunity to attempt to form a government. That policy position is relatively closer to the policy positions of the other two parties, the farther apart those positions are.

In a government formation process with a fixed order of selection involving all three parties, a political equilibrium has the property that the parties strive to serve the interests of their supporters by choosing policy positions which maximize their representation. In the political equilibrium, parties' policy positions are symmetric about the center and dispersed.

This paper has shown that multidimensional models of political systems with an institutional structure consisting of a proportional representation election system, a parliamentary government, and a government formation process can yield equilibrium party policy choices and corresponding voter support of parties. Since a political equilibrium depends on the government formation process, the theory indicates that empirical studies should give attention to the nature of that process. Furthermore, since party policies depend on the distribution of voter preferences, data on voter preferences should be linked to data on party positions.

The theory presented here includes a number of specifications that warrant generalization and extension. The principal specifications that warrant generalization include the dimensionality of the policy space, quadratic preferences, parties represented as unitary actors, and actual policies that can differ, because of imperfect implementation, from those agreed upon during government formation. The principal extensions include repetition of the model that would allow parties to develop reputations limiting their policy choices but enhancing their electoral prospects, elections in which voters form expectations based on the past performance of parties, opportunities for governments to fail, and entry by new parties.

APPENDIX

With a uniform distribution on a circle, the aggregate utility $v_3(w;\, S_3)$ of the members of party 3, for example, is

$$v_3(w;\, S_3) = -\int_{-x^*}^{x^*} \int_{as_1+h}^{\sqrt{1-s_1^2}} [(w_1 - s_1)^2 + (w_2 - s_2)^2]\, ds_2 ds_1, \qquad \textbf{(A-1)}$$

where, as illustrated in Figure 2, S_3 is identified by boundaries with slopes $a = (\sqrt{1 - x^{*2}} - h)/x^*$ and $-a$, the voter who is indifferent between supporting party 3 and either of the other two parties, is located at $(0, h)$, and (x^*, y^*) is the most distant ideal point for a voter indifferent between parties 1 and 2, where $y^* = ax^*$. The aggregate utility in equation A-1 may be expressed as

$$v_3(w;\, S_3) = -N_3\, [(w_1 - t_{31})^2 + (w_2 - t_{32})^2] + m_3,$$

where

$$N_3 = |\, S_3 \,| = \sin^{-1}(x^*) - hx^* \qquad \textbf{(A-2)}$$

is the number of voters who support party 3. The constant m_3 is

$$m_3 = 2\int_0^{x^*} \left[s_1^2\, (\sqrt{1 - s_1^2} - as_1 - h) + \frac{1}{3}\, ((\sqrt{1 - s_1^2})^3 - (h + as_1)^3) \right] ds_1,$$

and the party ideal point, the centroid of the set of voters supporting party 3, is $t_3 = (t_{31}, t_{32})$, where $t_{31} = 0$ and

$$t_{32} = \frac{2x^*}{3N_3}\, [1 - 2h^2 + h\, \sqrt{1 - (x^*)^2}\,].$$

The ideal point t_3, N_3, and m_3 are functions of x^* and h. For the case illustrated in Figure 2, if $h = 0$ and $x^* =$

$\sqrt{3}/2$, party 3's ideal point is $t_3 = (0, \sqrt{3}/\pi)$. The centroids for parties 1 and 2 are $t_1 = (3/2\pi, -\sqrt{3}/2\pi)$ and $t_2 = (-3/2\pi, -\sqrt{3}/2\pi)$, respectively.

The preferences of a party can be expressed as the average utility Av_3 of its members as in equation 2, which is given by

$$\begin{aligned} Av_3(w; S_3) &\equiv \frac{v_3(w; S_3)}{N_3} \\ &= -[(w_1 - t_{31})^2 + (w_2 - t_{32})^2] - \frac{m_3}{N_3}. \quad \textbf{(A-3)} \end{aligned}$$

The first term in equation A-3 is $u(w; t_3) = -[(w_1 - t_{31})^2 + (w_2 - t_{32})^2]$ and is used in my earlier government formation model (Baron 1991). The second term is independent of the policy w but depends on x^* and h and hence on the set of voters that support the party.

To relate the policy position to the party ideal point, the parameter h in equation A-1 will be expressed as a function of γ. To determine which voters support which parties, spatial preference theory can be used. For $(x, y) \in S$, the bisector line $y(x; \gamma)$ to the line connecting b_3 and b_1 can be expressed as

$$y(x; \gamma) = h(\gamma) + c(\gamma)x,$$

where

$$h(\gamma) = \frac{D\left(\gamma^2 - \frac{1}{2}\right)}{2\sqrt{\gamma^2 - \frac{1}{4}}} - \beta D \text{ and } c(\gamma) = \left(2\sqrt{\gamma^2 - \frac{1}{4}}\right)^{-1}.$$

This line intersects the boundary of the circle at a point $(x^*(\gamma), y^*(\gamma))$ indicated in Figure 2. The coordinates of that point are

$$x^*(\gamma) = \left(\frac{1}{8\gamma^2}\right)\left(-h\sqrt{\gamma^2 - \frac{1}{4}} + \sqrt{h^2\left(\gamma^2 - \frac{1}{4}\right)(1 - 64\gamma^2) + 64\gamma^2\left(\gamma^2 - \frac{1}{4}\right)}\right)$$

and

$$y^*(\gamma) = \sqrt{1 - (x^*(\gamma))^2}.$$

The vote for party 3 depends on its policy position, since both x^* and h are functions of γ. The vote share p_3 of party 3 expressed as a function of γ is, using equation A-2,

$$p_3 = \frac{N_3}{\pi} = \frac{1}{\pi}\left(\sin^{-1}(x^*(\gamma)) - h(\gamma)x^*(\gamma)\right).$$

For example, if party 3 were to locate at the midpoint of the line between the positions of the other two parties, voters with $s_1 \in [-\frac{1}{4}D, \frac{1}{4}D]$ vote for party 3. Moving too close to the ideal points of the other two parties can reduce a party's vote share. For example, if $D = 1$, party 3 receives 31.5% of the vote.

Notes

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1. See Schofield, Grofman, and Feld 1988 for an exposition of results about the existence of a core under supermajority rules.

2. Austen-Smith and Banks (1988) consider a one-dimensional policy space and three-party electoral competition in which sophisticated voters anticipate the policies of the governments that will be formed. The government formation process considered has a fixed termination point, at which time a caretaker government forms if none of the parties has been successful in forming a government on its first attempt. In the government formation processes considered here, there is no necessary termination point. In addition, the policies are multidimensional. As in Austen-Smith and Banks's model, the model takes the number of parties as fixed and does not allow for the possibility of entry. The political system considered here also differs from that of Cox (1990b) in that the policy space is two-dimensional and parties serve the interests of their supporters, rather than seek to maximize their vote share. Aldrich (1983) considers a two-party plurality system with voters who have spatial preferences and a cost of alienation. Equilibria are induced by modes in the (symmetric) distribution of ideal points.

3. Both approaches are consistent with minority governments in which the parties holding portfolios have a minority of seats in the parliament.

4. Entry of parties has been considered in Cox 1990b, Greenberg and Shepsle 1987, Greenberg and Weber 1985, Palfrey 1984, and Shepsle and Cohen 1990. Their results demonstrate that the existence of equilibria is sensitive to the nature of the electoral system and to the space in which parties can locate. Palfrey shows that if candidates locate in a one-dimensional space, there is an equilibrium in which two parties choose locations, and a third potential party is deterred from entering because it is certain to lose. Greenberg and Weber (1985) consider a political system in which the number of seats in the parliament is determined by how many candidates receive a fixed quota of votes. With a finite number of positions at which parties may locate and a finite number of voters, an equilibrium exists in which a number of parties choose locations and the others are deterred from entering because they cannot receive the number of votes required for a seat. Greenberg and Shepsle (1987) consider an electoral system in which there is a fixed number of seats and either a finite or an infinite number of party locations. They show that an equilibrium configuration of parties may not exist.

5. Although party leaders may serve their own interests, in addition to those of their supporters, the objective here is to investigate the policy preferences of parties, rather than the preferences of their leaders. Another interpretation of the model in terms of a party leader is given in the next section.

6. I have studied these processes in Baron 1991.

7. Factions within a party are thus not considered here. See Budge and Keman 1990, 32, for a perspective on the modeling of a party as a unitary actor.

8. See Black 1958; Downs 1957; and Enelow and Hinich 1984, 1990.

9. The platform, for example, may be subject to change only in the next party convention (Budge, Laver, and Strom 1991).

10. The effect of discounting will be considered.

11. This is a common assumption in models with a continuum of voters no one of whom can affect the election outcome. See, e.g., McKelvey and Ordeshook 1985.

12. A process with probabilities that change each round will be considered.

13. Note that here a government is identified by the parties that support the government proposal on investiture, rather than by an allocation of ministries. Minority governments, as identified by portfolios allocated to parties that only have a minority of seats, are thus consistent with this theory.

14. An equilibrium is said to be stationary if the players choose the same strategy in every structurally identical subgame. Stationary strategies are history independent.

15. If the process were characterized by other parties' having the opportunity to make a counterproposal before another selection round, the probability that a government would be formed on the first round is less than 1. See Baron and Ferejohn 1989. The government formation process could thus extend over more than one round. That process could also extend over several rounds if there were incomplete information about the preferences of a party.

16. I have characterized the equilibrium λ_{ij} in Baron 1991 and will not present it here.

17. The government policies in Austen-Smith and Banks 1990 and Laver and Shepsle 1990 are generally not on the contract curve for a coalition government.

18. In the terminology used by Cox 1990b, the first incentive is centripetal and the second is centrifugal.

19. See Baron 1991 for this analysis.

20. In the example illustrated in Figure 4 for the case in which parties 1 and 2 have fixed positions, party 3's optimal location is such that its vote share p_3 is less than $p^*(\gamma)$. If party 3 were to locate sufficiently far (relatively) from the other two parties and those parties were sufficiently far from the center, the vote share p of party 3 could be above $p^*(\gamma)$. In this case, parties 1 and 2 then make proposals to each other to form a government, so party 3 is less likely to be in the government because its position is extreme relative to the positions of the other two parties. The remaining question is whether there is an equilibrium in which $p > p^*(\gamma)$. Any equilibrium would require that b_3 be equidistant from b_1 and b_2. Unless, however, the three positions were symmetric, one party would have an incentive to change its position. Symmetry requires that $\gamma = 1$, but then $p^*(\gamma) = 1/2$. Consequently, the case in which $p > p^*(\gamma)$ is relevant only to the case in which two parties have fixed policy positions.

21. This example is used because it corresponds to the current model in which the smallest of these parties is equidistant from the policy positions of the other two parties and those parties have roughly equal representation. See Baron 1991 for a more extended analysis.

22. The optimal locations are determined by numerical evaluation.

23. This prediction is based on the maintained assumption of a uniform distribution of voter preferences on S.

24. Germany requires a threshold of 5% for representation in the Bundestag.

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David P. Baron is William R. Kimball Professor of Business, Economics, and the Environment, Stanford University, Stanford, CA 93405-5015.

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RECONSTRUCTIVE DEMOCRATIC THEORY

JOHN S. DRYZEK and JEFFREY BEREJIKIAN *University of Oregon*

While the idea of democracy has never been more universal or more popular, both democratic theory and the empirical study of democratic possibilities are in some disarray. We seek a productive reconnection of these two endeavors with democratic discourse through close attention to the language of democracy as used by ordinary people and political actors. Reconstructive inquiry determines how the individuals who are the potential constituents of any democratic order themselves conceptualize democracy and their own political roles and competences. We deploy an intensive method—Q methodology—for the study of individual characteristics, capabilities, and dispositions in combination with political discourse analysis. Four discourses are discovered in an analysis of selected U.S. subjects: contented republicanism, deferential conservatism, disaffected populism, and private liberalism. These results can be used to relate democratic theory to live possibilities in democratic discourse.

The triumph of democracy as an abstract idea is nearly complete, and further democratization is on the agenda worldwide. There is scarcely a political leader, analyst, dissident, or activist who would today justify his or her preferred polity in anything but the language of democracy. On the other hand, democratic theory is in some disarray. Liberal constitutionalists, pluralists, social democrats, Marxists, communitarians, feminists, libertarians, participatory enthusiasts and others all have their own favored forms of democracy. By any of their standards, *real* democracy is hard to find.

A cynical resolution of this paradox might be that "democratic theory is the moral Esperanto of the present nation-state system, the language in which all Nations are truly united, the public cant of the modern world, a dubious currency indeed" (Dunn 1979, 2). Here, we shall argue exactly the opposite, namely, that careful empirical study of the language of democracy used by the subjects of political regimes can help overcome some contemporary impasses in democratic theory. We combine political discourse analysis and a "Q-methodological" study of selected individuals in order to model discourses of democracy present in the United States and to ascertain how people conceptualize their political roles and competences. Four such discourses are identified: contented republicanism, deferential conservatism, disaffected populism, and private liberalism. Our approach is reconstructive in that it does its utmost to find its categories in how its subjects actually do apprehend the world, not in how the researcher expects them to do so. The findings thus generated should prove a more secure foundation for theorizing than the findings or assumptions generally deployed by democratic theorists, even as their applicability is constrained by the time and place of their generation.

Our findings can help test existing democratic theories because all such theories make claims about the dispositions of individuals who compose any actual or potential political order. But before scrutinizing existing theories in this light, we wish to emphasize the *generation* of theory in a reconstructive inquiry into how democracy can, does, and should work in terms of the categories and capabilities of the individuals who are themselves going to constitute any democratic order. The orientation for our efforts is provided by the strengths and weaknesses of existing approaches to the study of democracy.

THE CONDITION OF DEMOCRATIC THEORY

Democratic theorists have always sought empirical referents for theoretical propositions. In the last forty years, the tools of modern social science have made it possible to explore these referents in new ways. Unfortunately, the new, self-styled empirical theorists of democracy largely grounded their analyses in public opinion studies, and their critics would argue, the ideological status quo of U.S. politics. Thus, the appearance of these new social science tools coincided with the empirical impoverishment of the tradition of democratic theory, not its enrichment.[1]

Empirical democratic theory flowered in the 1950s and 1960s, when new technology, most notably the opinion survey, enabled summary judgment concerning the capabilities and dispositions of mass publics. The findings were chastening: for the most part, ordinary people proved politically ignorant, potentially intolerant, inconsistent and unstable in their opinions, and influenced by social and psychological forces beyond their conscious control. The lesson for empirical theorists such as Dahl (1956), Sartori (1962), and Eckstein (1966) was clear: the masses possessed, and should possess, no more than limited and indirect influence in politics. Existing institutions in the U.S. polity (and perhaps some others) seemed to provide the optimal level of mass access, especially through channels constituted by interest groups. Equally fortunately, mass publics in the United States and Great Britain proved to have just enough faith in their capabilities, tempered by just enough deference and parochialism, to accept and support the limited participation their systems allowed (Almond and Verba 1963). Thus, there seemed to be no difficulty in operationalizing "democracy" as a variable measured by degree of proximity to the Anglo–American ideal. The effect upon the degree of democracy of independent variables such as level of economic development (Lipset 1959) and mass attitudes (e.g., Prothro and Grigg 1960; Cnudde 1971) could then be investigated.

The years have not been kind to the empirical theory. Its micropolitical foundations have been questioned within the community of survey researchers (e.g., Fiorina 1981; Nie, Verba, and Petrocik 1976), though Kinder notes that these revisionist accounts involve no more than marginal adjustment to a dismal account of mass political capabilities (1983, 393–97). Perhaps the political events of the late 1960s and 1970s constituted more serious challenges to the empirical theory, calling into question both the stability of U.S. political institutions and their ability easily to accommodate new pressures and demands from below. The empirical theory of democracy seemed to run out of energy in this period, and some of its adherents changed their minds about actual and desirable democratic orders (e.g., Dahl 1982). But no new empirical theory arose to supplant that developed in the 1950s and 1960s; the weight of survey research findings still supports no alternative account; the "old" empirical theory is still entrenched in the textbooks;[2] and Sartori (1987), for one, remains unrepentant. So despite its manifest weaknesses, the empirical theory lingers. Its critics, for their part, have relished exposing its ultimately ideological commitments to the status quo in Anglo–American politics (e.g., Dunn 1979, 25–26; Pateman 1970, 1–17; Skinner 1973). These commitments mean that the empirical theory offers only premature closure to any debate on democratic possibilities.[3]

Given this record, it is perhaps understandable that those who continued the tradition of democratic theory turned away from empirical work to concentrate on more abstract visions. The products are idealistic and hopeful in, for example, the works of Arendt, MacPherson, Pateman, and Barber. The republics portrayed in the formalizations of liberal democratic theory by public choice scholars such as Arrow, Riker, Buchanan, and Tullock are more sobering. But in all its variety, democratic theory is today vulnerable to the charges of alienation leveled by Gunnell (1986) at political theory in general and withdrawal from the "great conversation" of democratic development (Ricci 1984, 319–23). Particular theoretical voices reach restricted audiences as the languages they speak become arcane and specialized. Think, for example, of the mathematical formalizations of public choice or the dense terminology of hermeneutics and critical theory. The alienation of *political* theory may be bad enough; but the alienation of *democratic* theory is worse because, ironically, any theorist of democracy is presumably postulating some measure of equality across speakers and listeners (however small this measure may be for liberal constitutionalists). Let us suggest, adapting John Dewey's remark, that the cure for the ailments of democratic theory is more democratic theory.

THE IDEA OF RECONSTRUCTIVE DEMOCRATIC THEORY

Reconstructive science concerns itself with the social competences of individuals and corresponding grammars of human interaction. Its categories are sought in its subjects, rather than specified by the analyst. As such, the reconstructive approach speaks to the problems generated by the "operationalism" that plagues survey research. Sullivan and his associates define this problem succinctly:

> The problem lies in investigating concepts in a particular way, by operationalizing them in an *a priori* manner that can severely and arbitrarily restrict the domain within which people can respond. Given this *modus operandi*, investigators are not likely to learn much from the subjects of their inquiry, other than whether people generally respond as predicted by researchers' hunches or theories. A richer process of learning and discovery by truly *listening to* respondents' views is precluded. (1990, 3)

Perhaps the best-known discussion of the idea of reconstructive science is Habermas' idealization of Lawrence Kohlberg's theory of moral development, Noam Chomsky's of linguistic competence, and Jean Piaget's of operational thought. Habermas (1979) claims that these theories are universally applicable and above reproach. Now, Habermas' essentialist argument on this score is almost certainly false. These three theories can be—and have been—disputed and, as such, merit a truth status no different from more familiar kinds of social-scientific theory (Alford 1985). Moreover, when it comes down to it, their categories are specified in the theorist's interpretation of some ideal subject, rather than by the subjects themselves. This idealization holds, too, for Johnson's (1991) rendition of game theory as a reconstructive science. On Johnson's account, game theory is not a positive, predictive science; rather, it tells us simply what strategic rationality *is* in particular contexts, which means that empirical test is not the key to determining the theory's veracity.

We intend to develop a truly reconstructive approach that lets subjects speak for themselves about their interactive competences, and the categories (in our case, democratic theories) that these competences help construct. Thus, in speaking of the competences of individuals, we are not holding them up against any measuring rod external to them. Our notion of reconstructive theory is, then, somewhat different than that of Habermas and Johnson; for we afford less license to the analyst to reconstruct the competence of some ideal subject. Our concern is with actual subjects, and we have no preconceptions as to the content of their competences.

The idea that the analyst should attend closely to subjects' own constructions of politics has in the past been advanced most forcibly in ethnographic studies involving intensive interviewing, notably those of Lane (1962) and Hochschild (1981).[4] While applauding the ethnographers' intent and recognizing their accomplishments, we try here to be still more truly reconstructive; for to make sense of the conversational data they collect, ethnographers must creatively impose coherence on subjects' orientations and dispositions. This creative act is inevitably personal, partly intuitive, and unconstrained. In contrast, the methodology followed here is explicit, public, constrained by statistical results, and replicable in its reconstruction and measurement of subjects' orientations, thus affording less interpretive latitude to the analyst. In addition, our methodology is quite capable of yielding surprising results that run directly counter to the institutions and expectations of the analyst. We do not claim that our kind of reconstructive inquiry is the only proper way to understand human political disposition and action but simply that it has distinct advantages over ungrounded philosophical assertion, unconstrained ethnographic interpretation, and restrictive opinion survey operationalization.

In doing democratic theory in reconstructive terms, our concerns lie in uncovering the *dispositions* of individuals (i.e., their opinions about matters of fact and value relevant to democracy) and their *self-described capabilities* as potential members of a democratic order. Dispositions and capabilities are of course intertwined, and we shall not attempt to measure them separately. It is the *interactive* competences of individuals that are our concern, and these competences have no existence independent of the individual's orientation to his or her social context. In other words, how individuals construct reality is inseparable from the roles they think they can, do, and should play in relation to that reality.

Q METHODOLOGY

Reconstructive democratic theory as proposed here makes use of the most fully-developed paradigm for the investigation of human subjectivity, *Q methodology*. We combine "Q" with a newer methodology, political discourse analysis. The two can be conjoined, inasmuch as both seek to model structures embedded in language.

Q methodology was invented by the psychologist William Stephenson (see, esp., Stephenson 1953). A Q study begins by modeling subjects in terms of their reactions to a set of statements about a given domain. Unlike more widely applied "R" techniques, such as survey research, Q is not concerned with patterns across variables but, rather, with patterns within and across individuals. To this end, the whole subject is modeled at once; a respondent's reaction to any one statement only makes sense in the context of his or her reactions to every other statement in the set. We compiled 64 such statements, which our subjects were asked to order in a quasi-normal distribution. The extremes of the distribution were coded +6 for *most agree*, and −6 for *most disagree*, with 0 indicating indifference. The ordering a subject produces is called a *Q sort*; it represents the subject's orientation to the domain in question.

This orientation is produced through reference to language the subject is likely to use and understand, not in terms of categories developed by the investigator. As Kitzinger puts it, "the theoretical basis on which Q methodology is founded relies on the axiom that researchers should acknowledge and present the reality constructions of different women and men without prejudging or discrediting them, and without insisting on the superior (more 'objective') status of the researcher's own construction of reality" (1986, 153). Q is therefore a reconstructive methodology, though an imperfect one because it is the researcher who defines the domain of study. In our case, we defined the democratic domain as concerned with the collective construction, application, distribution, and limitation of political authority.

Individuals' Q sorts can be compared using such statistical techniques as correlation, clustering, and (most commonly) factor analysis. As used with Q methodology, these techniques seek patterns across individuals, not variables. Thus, people who load highly on a factor reveal a high level of commonality with one another and dissimilarity with people who load highly on orthogonal factors (or negatively on that same factor).[5]

CONCOURSE THEORY AND POLITICAL DISCOURSE ANALYSIS

The statements that an individual is asked to order in a Q sort are drawn from a *concourse* of verbalizations in which that individual has some interest. A concourse is "the volume of discussion on any topic" representing an interplay or running together of positions, ideas, and opinions (Brown 1986, 58). More formally, a concourse can be operationalized as the population of statements about some topic. The analyst draws a representative sample of statements from the concourse at hand (in our case, democracy), with the intention of using them in a Q study to model the operant structure existing in that concourse.

Fidelity to our reconstructive principle requires that the statements selected by the analyst and presented to the subjects for ordering be drawn from those actually made by individuals involved with the concourse. We gathered about three-hundred statements from newspapers; magazines (ranging from the John Birch Society's *New American* to the leftist *Guardian*); voter's pamphlets; quotation dictionaries; the verbatim reports contained in the ethnographic studies of Lane (1962), Hochschild (1981), Bellah and his associates (1985), and Reinarman (1987); and discussion groups on democracy for which we recruited people through newspaper advertisements.[6] Again, our reconstructive commitment meant that we did not edit statements, beyond occasionally supplying the noun for *it*.[7] Some statements were ambiguous, but that is the nature of political language. Ambiguity is resolved by each subject and reflected in his or her placement of a statement in relation to other statements. Of the 64 statements we eventually used, 20 came from ethnographic studies, 17 from magazines, 15 from our discussion groups, 5 from newspapers, 4 from voters' pamphlets, and three from quotation dictionaries. In keeping with our earlier point that reconstructive science treats competences and orientations as inseparable, we sought statements that refer to the individual making them, as well as to the world "out there." An example of the former kind of statement is "I don't feel that I'm that knowledgeable about things. If I knew a lot more actual facts and everything, then I might be able to strike up a conversation about politics."

To reduce the 300 statements to a manageable 64, we needed a procedure as independent as possible of our particular interests. Complete independence is, of course, impossible, given that we had to make judgments in selecting statements in the first place (based on whether they belong in the democratic concourse); but Q moves further in this direction than such procedures as survey research. Q-methodologists often use a cell structure as a heuristic device for selecting statements. Brown justifies a rough-and-ready approach to cell sampling on the grounds that "there is no standard Q sample for a concourse. Any suitably comprehensive sample is adequate for purposes of experimentation" (1986, 73). We believe we can do better when it comes to studying essentially *political* concourses (such as that pertaining to democracy) for the following reasons.

A concourse about such a topic as democracy will normally contain several *discourses* (so termed in the literature). A discourse, by definition, "embodies a shared set of capabilities which enable the assemblage of words, phrases, and sentences into meaningful 'texts' intelligible to readers or listeners" (Dryzek 1988, 710, following van Dijk, 1985). It is in this sense, for example, that Edelman (1977) analyses the discourse of welfare state administrators and what this discourse implies about the causes, consequences, and inevitability of poverty. In the context of democracy, one might expect, say, liberal individualists and communitarians to inhabit different discourses, across whose boundaries one would expect incomplete intelligibility. Thus, the same words, phrases, or statements (e.g., "democracy demands diversity") may mean different things in different discourses.

We intend to make sure that the key elements that define discourses are indeed generated and identified. What, then, is the character of these key elements? And how can one ensure that a Q study captures them? Our answer is that one should seek the quintessentially *political* aspects of discourses and so engage in what Seidel (1985) and Alker and Sylvan (1986) call "political discourse analysis." The vital elements of a political discourse, as formalized by one of the present authors elsewhere, are as follows:

> 1. An ontology, or set of entities, is recognized as existing (e.g., individuals, classes, nations, genes, interests).
> 2. Degrees of agency are assigned to these entities. Some will be construed as autonomous subjects (e.g., classes, by Marxists), others will be seen as objects, which can only be caused (e.g., the state, seen by vulgar Marxists as fully controlled by the dominant class). In other words, some entities can act, others can only be acted upon.
> 3. For agents, some motives will be recognized, others denied. Relevant motives might include material self-interest, civic virtue, impartiality, survival, and so on.
> 4. There are conceptions of natural and unnatural political relationships. Such relationships include, most importantly, taken-for-granted hierarchies based on age, education, birth, gender, wealth, social class, and so on. (Dryzek 1988, 711)

In analyzing a concourse—and resolving it into its component discourses—statements should be sought in all four categories. Thus, these four categories constitute one dimension of a cell structure for sampling a concourse.

The other dimension of the cell structure is less crucial and more heuristic. Bearing in mind that any political discourse is going to embody certain *claims* about the world, one can follow Toulmin's (1958) classification of the sorts of claims that can be made in arguments. These types are definitive (concerning the meaning of terms), designative (concerning questions of fact), evaluative (concerning the worth of something that does or could exist), and advocative (concerning something that should or should not exist). Combining these two dimensions yields a four-by-four matrix for sampling a concourse, as shown in Table 1.

A statement from cell 2 is "Community means people who interact at a personal level; have shared identity, values, and traditions; and possess the power to make decisions about their common values." One from cell 7 is "All governments, and the elites that live off them, want to control public opinion." Another, from cell 12, is "The way that democracy is manifested in our culture, some people are more equal than others. It becomes a double standard very quickly." And from cell 13, is "Religion should be banished from politics because a democratic politics is based not on truth but on justice." After eliminating statements with no clear fit in any cell, we took four from each cell at random, yielding 64 statements for the Q sample.

This sample was administered to 37 subjects. Our guiding principle in subject selection was diversity, so that if a discourse (however marginal) existed, we would be as likely as possible to locate it. Some of our

TABLE 1

Matrix for Sampling a Concourse

	DISCOURSE ELEMENT			
TYPE OF CLAIM	ONTOLOGY	AGENCY	MOTIVATIONS	NATURAL
Definitive	1	2	3	4
Designative	5	6	7	8
Evaluative	9	10	11	12
Advocative	13	14	15	16

Note: The numbers identify the cells from 1 to 16.

subjects were identified on the basis of known political commitments: we sought peace, civil rights, environmental, labor union, religious, and business activists, Republicans, Democrats, libertarians, liberals, conservatives, socialists, and feminists. The remainder of our subjects were chosen with an eye to maximizing social variety. We sought white- and blue-collar workers, the retired, rich and poor, males and females, voters and nonvoters, members of different ethnic and racial groups (African-American, Asian, Hispanic, and white), adherents of different religions (and of none), urban and rural residents, and residents of different states. Limited resources meant that we could not contact subjects from across the United States. Our subjects are concentrated in California, Oregon, Washington, Wisconsin, West Virginia, Maryland, and North Carolina.

Q is an intensive form of analysis and always works with small numbers of subjects. Thus, one can never claim that one's subjects are statistically representative of some larger population. But that is not the point, for the patterns that Q-methodologists find in some small group of subjects can be expected to reflect or intimate the structures existing in some larger population of subjects (Brown 1980, 66–67). Thus, a discourse that one identifies within a small-*n* Q study will generally prove a genuine representation of that discourse as it exists within a larger population of persons; and *this* is the kind of generalization in which we are interested. To put it another way, our units of analysis, when it comes to generalization, are not individuals but discourses. And we soon get to a point where adding individuals to a study does not yield any new information unless the extra individuals are truly different—another reason for stressing diversity in subject selection.

Like most successful sciences, Q has confidence in its individual observations (in contrast to large-*n* statistical studies, which utilize large numbers precisely because they have little confidence in individual observations). We cannot say just what proportion of any larger population subscribes to the discourse in question—though that question could be answered easily (if expensively) by a sample survey (see Sullivan et al 1990). Let us now turn to the discourses we have identified.

RESULTS

We report here four discourses identified through factor analysis of the Q sorts of our 37 subjects.[8] An idealized Q sort can be computed for each discourse; this represents how a hypothetical individual loading 100% on a factor would order the 64 statements. In Table 2 we report the scores of all 64 statements on the idealized Q sort for each of the four discourses (factors A, B, C, and D).

The most parsimonious form in which to present the four discourses is in terms of a label and a narrative for each. But we must stress that these narratives are not constructed by simply cutting and pasting statements with extreme scores on each factor; for the narratives must also take into account how statements are placed relative to one another in each discourse (apparent paradoxes may require resolution) and the comparative placement of statements in different discourses. In common with interpretive social science more generally, this exercise assumes that each discourse represents a coherent point of view. Thus, any apparent internal inconsistencies require explanation, rather than just dismissal or criticism. The four discourses, each of which embodies a theory of democracy, are as follows. (The numbers in brackets refer to statement numbers in Table 2.)

Contented Republicanism (Factor A). We live in a democracy, which is fortunate because democracy is without doubt the best form of government (40, 60). Democracy is a way of life, not just a political system; it is bound up with our freedoms, and, though fallible, can correct its mistakes (3, 1, 21, 35). Thus, there is no need to fear an excess of democracy or to fear for the future of democracy (53). This future depends on the citizenry remaining active and involved in politics (8). There is no difficulty here; for politics is an uncomplicated activity in which everyone can and should be involved, though it would be good if the poor and worse-educated were to participate more (39). Political equality is important and easily achieved and does not require social and economic equality (15, 61, 30). There is such a thing as the people, and power is in its hands (24). This is a

TABLE 2

Statement Scores on Each Factor

	FACTORS			
	A	B	C	D
1. I don't think freedom has anything to do with democracy.	−4	0	−6	+2
2. Democracy is never easy to define. The meaning of the word changes with the vagaries of time, place and circumstance.	0	+4	0	−2
3. Democracy is a governmental form, not necessarily having anything to do with society as a whole or the way society works.	−4	−2	−3	+1
4. Democracy is an empty vessel into which one can comfortably fit socialism as well as freedom, a corrupt society as well as a healthy people.	−1	+1	−1	+4
5. Community means people who interact at a personal level; have shared identity, values, and traditions; and possess the power to make decisions about their common values.	+4	+4	0	+4
6. Citizenship is a man's basic right for it is nothing less than the right to have rights.	+2	−1	0	−1
7. Any citizen who can read and wants to take the time can make a good decision among candidates for an office.	+1	−3	−1	0
8. Mass democracy depends on an informed and active citizenry.	+6	+3	+3	−2
9. To try to find the public good, I would try to ask questions about how this or that would affect the community twenty five years from now. Not whether such and such a regulation will affect somebody's else's pocketbook.	+1	+5	+1	−1
10. If I wanna say my mind that's fine. But I also have the right not to listen, and that's what the first amendment gives me.	+1	0	0	+5
11. When the president does it, that means it's not illegal.	−6	−6	−5	−5
12. Democracy means my right to choose what's best for me and the public at large.	+4	0	+4	0
13. Work and family are the center of our lives, the foundation of our dignity as a free people.	+2	−1	−3	+6
14. Democracy demands diversity.	+5	−1	+2	+1
15. Men are not created equal. They have different intelligence; they're born into different situations. What "all men are created equal" basically means, of course, is whether you're white or black or poor, you should have your equal say, your equal education, be treated equally.	+5	+5	−2	+1
16. I think the rights of the individuals add up to majority rule.	+1	+1	−4	+2
17. You can have a socialist society and have the government elected in a democratic election.	+1	+1	+2	+5
18. A market economy, a democratic legislature, an independent judiciary, and constitutionally guaranteed rights are the only proven foundations for sharing power.	+1	−2	−1	+3
19. Democracy sure does not exist in its pure form today.	0	0	+1	+3
20. I have a big problem identifying with hundreds of millions of anything—people, flowers, cars, miles. I can see the community around me.	−2	0	−2	−3
21. We the people have not chosen the freedoms that we have.	−3	−1	−2	+2
22. I don't feel that I'm that knowledgeable about things . . . If I knew a lot more about actual facts and everything, then I might be able to strike up a conversation about politics.	−3	+1	−4	−4
23. Not everyone is going to be represented, there are always those people who fall through the cracks.	+1	+6	0	+3
24. If things don't get done, it's because the people don't go out and do it. The power is with the people.	+3	−1	+4	0
25. I'm interested in the national economy and our defense ability, not all these crappy issues like human rights.	−6	−5	−4	−4
26. I don't think it's possible to have a democracy of three people without greed being the main factor.	−4	+1	−4	−2
27. In essence, the government doesn't really care about what I say.	−2	+2	−1	−1
28. All governments, and the elites that live off them, want to control public opinion.	−1	+1	+4	+1
29. Elected officials are better able to make decisions for voters than voters themselves.	−3	−4	−5	−6
30. Now we are pretty much all equal. I don't think the people stand for NOT being treated equal any more.	−2	−6	−6	−3
31. The daily lives of all citizens are deeply affected and changed by the decisions of government.	+3	+6	+2	+6
32. I can get pretty confused when I listen to political speeches. A guy gets up and sounds like he's the best there is. The next guy gets out and makes a bum out of him. You don't know who to believe.	−2	+5	−1	−1
33. A free press protects our basic liberties by serving as the watchdogs of our nation.	+5	−2	+5	−2
34. If I can find something that's good for the average person, that's good for democracy.	−1	−2	−2	−1
35. Democracies can make mistakes, but they have the unique advantage of a way to make corrections before it is too late.	+3	−5	+1	+1
36. Government can't be too powerful because the government is the people.	−3	−3	−2	−3
37. A lot of government's problem is too many chiefs.	−1	0	0	0

TABLE 2

Statement Scores on Each Factor *continued*

	FACTORS			
	A	B	C	D
38. Anybody can stand on the street corner and under the Constitution voice pretty much anything, and that's problem.	−5	−4	−5	−3
39. We should be critically concerned about the drop-off in democratic participation in the United States, and about who is voting (the educated and economically well-off) and who is not voting (the poor, the young, minorities, and the least educated).	+3	+2	+6	+3
40. America has deteriorated in the direction of a democracy as we have become a government of men instead of a government of law.	−3	0	+3	+3
41. In a systematic way, the rights of individuals have been curtailed in this country, not for the benefit of the whole nation, but in conformity with a narrow interpretation of a vocal conservative minority.	0	+2	+5	−1
42. A major problem with democracy is that many people do not know what they want.	−1	+2	−3	0
43. Violence is a just and legitimate reponse to the economic, political, and military violence employed by the state.	−5	−2	+1	−5
44. I'd like to hear all points of view. Sometimes it's confusing, but I'd rather like that.	+2	+3	+1	−3
45. The way that democracy is manifested in our culture, some people are more equal than others. It becomes a double standard very quickly.	+2	+3	+6	0
46. I don't think social class is important.	0	−4	−2	0
47. The government is like a domineering mother. It take away all the people's incentives and tries to do everything for them. You know what it's like for children who have been dominated all their lives by a strong, powerful mother. They become near-vegetable cases. It's the same with government.	−4	−3	0	+4
48. The impersonal hand of government can never replace the helping hand of a neighbor.	+1	+2	+3	+5
49. Politicians should not be allowed to get away with lying as a form of free speech.	+4	+3	−1	0
50. Religion should be banished from politics because a democratic politics is based not on truth but on justice.	−2	−1	0	−4
51. All voters should be allowed as much information as possible to make their choices. Any attempt to regulate political literature and speeches is a move against our first amendment freedoms and toward totalitarianism.	+6	+4	+2	+2
52. Our system of social justice must be, and is, based upon natural human rights.	+3	−5	+1	+1
53. The future of democracy? Bleak. It's just gonna get worse, unless things get really bad and people start protesting again.	−2	−1	+3	+2
54. If everyone put what money and/or time they could into pollitical campaigns and causes, we would all be better off.	0	−4	−1	−5
55. If you want the state off your back, put your feet to the pavement.	0	−3	+2	−1
56. We should relocate power away from elites and the federal government and return it to the community level.	−1	0	+1	+4
57. You're part of society. Everybody has to do something to help society.	+2	+4	+4	−2
58. I think we should take a good look at future candidates and ask questions about him that are for the good of the people and not just big business.	+2	+3	+5	+1
59. All that is needed is for governments to be honest, for those governments need the people.	0	+1	−1	−2
60. Democracy is best, it's something we should strive for.	+4	+2	+3	−1
61. We can build a world where it is self-evident that all people are created equal.	−1	−3	+1	+1
62. Under communism the state would supply all the money for you. Everyone would be working for the state, there wouldn't really be no rich people in it. That would be good, if everyone is on kind of an equal level.	−5	−2	−3	−6
63. The family—like most traditional institutions—simply needs to be made more democratic.	0	−1	−3	−4
64. The perfect society is everybody living in accord, going about their own business and having separate interests, but that hopefully these interests all mesh nicely, they aren't bumping into each other and conflicting.	−1	+1	+2	+2

diverse society, but that is good for democracy (14). It is our common citizenship that unites us, not communal traditions; citizenship is basic to our way of life (6). Politics need not be based on greed or self-interest; for democratic debate can help establish an identity between what's good for me and what's good for society (26, 12). The importance of this debate means that there should be no restraints on the availability of information; a free press is crucial, and we should not tolerate lying in politics (51, 33, 49). Thus, government should be seen not as an adversary but as the institution in which our citizenship is embodied—though, of course, government can become too big and intrusive and so may need constitutional restraint (47, 36). The government does listen to me, and there are few limits to my capacity to exercise political influence (27).

Deferential Conservatism (Factor B). Politics is only for the few. Not everyone is capable of making good decisions, people don't know what they want, and not everyone can be represented (7, 42, 23). Thus, human frailty makes democracy problematical and hard even to define (26, 42, 2). While this difficulty in defining the concept means that democracy does not necessarily bring bad things, democracy is not particularly valuable or effective, either (12, 35, 60). Certainly, democracy should not pervade society; and it doesn't necessarily have much to do with real freedom (1). It is undesirable for people to get any more involved in politics; citizen activism is not a good thing (54, 55). Nor are the liberal values of a free press, independent judiciary, social justice rooted in basic rights, or the market very attractive (18, 33, 52). To me, politics is remote and confusing (22, 32, 2). But this does not matter, because government does not listen to people like me anyway (27, 32). However, there is no need to fear government; we should rely on elites to govern and hope that they are honest (47, 55, 49). Such elites will be able to look out for the long-term interest of society, which matters more than short-term economic concerns (9). This is an unequal, class-divided society; and little can or should be done about that (15, 30, 46, 61).

Disaffected Populism (Factor C). We do not live in a democracy, as power is in the hands of conservative, corporate elites and a government that represses the people (41, 28). This is a highly unequal society, especially in terms of political influence; and calling it a democracy does not change that (30, 45). Over time, democratic control has deteriorated; and the future of democracy is bleak unless people wake up and do something about it (39, 40, 53). The freedoms that are central to democracy have been curtailed (1, 41). It is up to the people to rise up and challenge this situation; they have the capacity to do so (55, 24, 42). The power of big business and corporate elites should be restricted (58). Thus, equality and true democracy are possible and desirable even though we are very far from them today (8, 15). Ordinary individuals are well motivated; and they should attend to politics, rather than be preoccupied by work and family (26, 13)—though politics cannot, and should not, be all-consuming. Community does not matter much, and so it does not matter at what level of government power resides (5, 56). More important is the people acting together—confronting government, rather than occupying institutional roles within it. In this critical sphere, a free press is essential (33). One should not necessarily condemn political violence when it is undertaken by the oppressed (43). I have complete confidence in my own political capabilities, and those of citizens more generally (22).

Private Liberalism (Factor D). We do not live in an especially democratic society, and not everyone can be represented (19, 23). But this is no cause for concern, for democracy is of no particular value, given that it can encompass both desirable kinds of government and undesirable forms, such as socialism (60, 17, 4). Government itself is something that need not be central to society (3). Moreover, society should not be central to individuals, who do not have to consider themselves part of, or contribute to, society (57). Individuals should be free to pursue their own interests, but democracy will not guarantee that freedom (1, 21). It is the private realm which really matters—work and family are the most important things in life, and one should rely on friends, neighbors, and the market, rather than government (13, 48, 18). Interests defined in the private sphere are paramount, and that is why (for example) it is fine for religion to motivate political action (50). Government, however democratic, has intruded too far into this private realm; government should be small and subject to separation of powers and constitutional restraints (31, 47, 18). Further, power should be moved back to communities, away from the federal level (56). Active citizenship is not important or desirable, and individuals have every right to ignore politics; it would be undesirable for people to put more time and effort into politics (8, 10, 54). I'm not interested in public debate, and so do not care unduly about a free press (44, 33). This lack of interest is not because I lack confidence in my own capabilities, but because politics is not central to life (22).

These, then, are our four discourses of democracy. The ontology, ascription of agency, motivation, and natural relationships associated with each discourse are further—and radically—summarized in Table 3. The four discourses are divided on the basic issues of who or what constitutes a recognizable political entity, which of these entities can act, their reasons for action, and the kinds of relationships between entities that can exist. Thus A's contented republicanism recognizes an unproblematic harmony encompassing active individuals and an active people, private and public realms. Consequently, A's democratic enthusiasm is unbounded, treating political equality as an accomplished fact. But this enthusiasm is not quite Panglossian, inasmuch as it seeks still more extensive participation, to which it perceives no structural impediments. Politics for contented republicanism is a matter of honest talk about the public good, coextensive with individual activity and social life.

Though both A and B accept the broad political status quo, in other respects they could hardly be more different. B's deferential conservatism constitutes a discourse that accepts that political hierarchy is natural and whose commitment to democracy is correspondingly problematical. While B seems to assent to democracy as an abstract symbol, its more specific judgments are not easily described as democratic on any definition of the term. Political agency for B can only be a property of elites; and this rules out any measure of active citizenship, political equal-

TABLE 3

Analysis of Four Discourses of Democracy

	DISCOURSE ELEMENT			
DISCOURSE	ONTOLOGY	AGENCY	ASCRIBED MOTIVATION	RELATIONSHIPS SEEN AS NATURAL
A. Contented republicanism	the people, individuals	everyone	public good	political equality, harmony between public and private interests
B. Deferential conservatism	society, elites	elites	mysterious	hierarchy
C. Disaffected populism	corporate/governmental elites, the people	presently elites, properly the people	elite-own interest, people are properly radical	presently class conflict, properly radical equality
D. Private liberalism	individuals, government	individuals (but not necessarily in politics), government	material self-interest	separateness of individuals, conflict between individuals and government

ity, and public participation. Individuals loading highly on B have little faith in their own competence as political actors. Perhaps the most that can be said for B's democratic commitment is an expectation of honesty on the part of elites and a desire to be able to listen effectively to political communication. B cares little even for liberal constitutionalism and liberal freedoms.

C's disaffected populism is as keen on political equality as A's contented republicanism. However, C believes that society is currently class-divided and repressive, such that mass action against political–economic elites is called for. C is an insurgent democrat's discourse, tinged with liberalism in its commitments to human rights, a free press, free speech, and individual differences but less liberal in its ascription of agency to the people. The popular struggle C seeks must contend with not only a repressive system and the forces that perpetuate it but also with two discourses that would oppose such a struggle. Nor is the tactical question here made any easier by the fact that this opposition would come from the very different perspectives of the deferential conservatives and the private liberals. The contented republicanism of factor A would not necessarily oppose all of the ends of C's struggle, but it would question the need.

The private liberalism of discourse D has a guarded view of democratic possibilities, stemming from the fact that it sets individuals apart from one another (partially excepting friends and neighbors) in pursuit of personal advantage and in conflict with government. As a result, D is energized mostly by the need to establish and protect the private realm against social sanction and government interference. Like B, D is not interested in extensive political participation, though for different reasons. B's deferential conservatives have little faith in the competence of themselves and others, whereas D sees no limits to this competence but believes it should be deployed in nonpolitical pursuits. D has much less faith than B in the benevolence of political elites and so accords far greater importance to constitutional forms that might constrain elites.

We should note further that all four discourses are united on a number of points. All disagree strenuously with Richard Nixon's remark that "When the president does it, that means it is not illegal" (11). None claims to be hardheaded enough to confine its concerns to defense and economics, as opposed to social justice (25). None has particular faith in democratic representatives being any more capable of making choices than ordinary voters (29); thus, none is really a discourse of *representative* democracy. None wants to restrict freedom of speech or information (38, 51); and all profess to care about declining voting participation (39)—though B's concern is marginal. All are anticommunist (62). None believes we live in a truly equal society (30). None has unlimited faith in the benevolence of government (36).

Do the four discourses we have identified exhaust the range of possibilities? An objection here might be that discourses are conditioned by the institutional and cultural settings in which they arise. Lurking in the background here is the possibility of ideological hegemony and concomitant foreclosure of democratic possibilities. However, we believe our results demonstrate that such hegemony does not exist. And there is a sense in which our methodology is *de*constructive (in the sense often used in literary criticism), in that it problematizes and picks apart seemingly consensual concepts and discourses.[9]

Our results do not allow us to generalize about how the discourses relate to social and political characteristics in the population at large. However, looking only at our subjects, it is striking that the four discourses cut across standard social and political categories. Table 4 reports the loadings of our 37 individuals on the four factors. Individuals with significant (at the .01 level) loadings on contented republicanism included a "conservative" real estate

TABLE 4

Subjects' Factor Loadings

SUBJECT SELF-DESCRIPTION	FACTOR A	B	C	D
1. F, Tree farmer, independent	49*	15	73*	1
2. M, Real estate developer, conservative	68*	18	29	26
3. F, Union representative, Democrat	27	2	60*	39*
4. F, Public relations, Libertarian	15	−4	20	56*
5. F, Administrator/consultant, liberal/feminist	65*	16	38*	11
6. F, Teacher/freelance artist, concerned/confused	3	61*	15	32
7. M, Self-employed, free-market socialist	24	22	26	26
8. F, Accountant, liberal Republican	36*	42*	14	34*
9. M, Army sergeant, open-minded	65*	10	13	21
10. M, Manager, conservative Republican	52*	19	33	27
11. M, Office manager, liberal	68*	20	−1	17
12. M, Computer programmer, moderate–liberal Republican	57*	25	16	20
13. M, Health care provider, independent	58*	−10	43*	32
14. F, Waitress, not political	20	42*	−4	47*
15. M, Bookstore owner, anarchist	10	5	46*	13
16. M, Pharmaceutical salesperson, Republican	37*	10	66*	7
17. F, Clerical, conservative	16	39*	1	36*
18. M, Union leader, liberal Democrat	64*	14	21	20
19. F, Office manager, radical	−11	35*	44*	13
20. M, Retired, open-minded	65*	7	20	42*
21. F, Forest Service employee, "don't know nothing"	11	21	47*	69*
22. F, No employment, indifferent/ ignorant	43*	31	20	14
23. F, Craftsperson, liberal	53*	46*	25	27
24. F, Insurance adjustor, conservative	73*	27	19	22
25. M, Accountant, Democrat	14	64*	18	−12
26. F, Student, liberal Democrat	27	7	24	39*
27. M, Contractor, liberal	18	24	0	42*
28. F, Clerical, conservative	40*	27	11	17
29. M, Student, moderate Democrat	51*	1	18	9
30. F, Student, socialist liberal	27	8	56*	21
31. M, Computer analyst, constitutional conservative	12	8	8	51*
32. F, Student/teacher/organizer, democratic socialist	51*	24	46*	−5
33. M, Store detective, slightly right of center	51*	−4	24	35*
34. M, Retired engineer, independent conservative	49*	49*	5	25
35. M, Bowling alley employee, moderate liberal	32	48*	32	7
36. F, Homemaker, conservative	17	12	4	33
37. F, Secretary, liberal	46*	38*	−4	17

Note: M = male, F = female.
*$p \leq .01$.

developer, a "liberal/feminist" administrator, an "open-minded" army sergeant, a "liberal Democratic" union official, and a "slightly right of center" store detective. Those loading heavily on deferential conservatism included a "concerned but not active, confused" artist–teacher, a "Democratic" accountant, an "independent conservative" retired engineer, and a "moderate liberal" bowling alley employee. Disaffected populism attracted a "politically independent" tree farmer who volunteers for the League of Women Voters, a (different) "Democratic" union official, an anarchist bookshop owner, and a "Republican" medical salesperson. And private liberalism drew a "libertarian" public relations worker, a "non-political" waitress, a Forest Service employee who "don't know nothing about politics," and a "constitutional conservative" computer analyst. (These quoted political descriptions are as supplied by the subjects themselves.)

DEMOCRATIC DISCOURSE AND DEMOCRATIC THEORY

What then does all this tell us about the possibilities for different kinds of democracy? We can make no sweeping generalizations about the characteristics, capabilities, and dispositions of the masses of the sort with which the empirical theorists of democracy dismissed participatory possibilities and argued the inevitability of a plural, elitist, and limited democracy. No generalization of this scope can survive our four discourses.

A theory of democracy is viable to the extent that there is a discourse or combination of discourses to which it can relate. The absence of any such relation undermines the model of democracy at hand *precisely because the model claims to be democratic,* that is, meeting with some degree of mass approval.[10] This does not mean that our four discourses define rigid boundaries outside of which fruitful democratic theorizing cannot occur; for the presence of several individuals with significant loadings on two factors indicates that in contrast to the world as portrayed by poststructuralists, there is not necessarily radical discontinuity across discourses: such individuals have access to both of the discourses in question. Assuming that this pattern is not just a methodological artifact, it provides some hope for theorists interested in articulating alternative or novel democratic theories, because it suggests that potential audiences are not completely imprisoned by prevailing discourses. But this possibility does not exempt theorists from the need to relate their schemes somehow to existing discourses.[11]

In this vein, we argued at the outset that our findings could shed empirical light on existing theories of democracy. To begin, we may note that none of our four factors really represents the liberal democratic model often thought basic to the political culture and regime of the United States. Liberal democracy would probably feature some mix of A's contented republicanism and D's private liberalism; but note that hardly any subject loads heavily on both A and D (see Table 4). Liberal democratic discourse may exist, but we find it less striking than the four discourses we have identified.

Other established theories fare better in light of our results. Thus, our discourse A bears some resemblance to the classical republicanism that began in ancient Rome, flickered in Renaissance Florence and seventeenth-century England, and influenced the U.S. founding. This tradition is conventionally defined in terms of commitments to a mixed government (or separation of powers); the rule of law; and, above all, an active, virtuous citizenry (see, e.g., Canovan 1987). Classical republicanism as a distinctive tradition waned by the twentieth century, though its emphasis on positive freedom, civic virtue, and personal fulfilment through public life reappears in the work of Hannah Arendt in the 1950s and 1960s. Arendt's political philosophy is as much a lament for republics past as a hope for republics future. A more applied republicanism (if not by that name) has been advanced in Kelman's (1987) highly optimistic interpretation and positive evaluation of U.S. politics. Our contented republicanism shares his assessment that politics is already pervaded by public spirit and that all that needs to be done is make the most of it.

Discourse A and classical republicanism both value active citizenship and public life, and both abhor the politics of self-interest. However, unlike the classical tradition, our contented republicanism evidences no fear of democratic excess. This fear underpins the classical stress on mixed government, which, especially in James Madison's corrective to the tradition, puts active citizenship in a decidedly unheroic and subordinate place. In contrast, our contented republicanism favors a degree of popular activism greater than even Arendt would seek, given her belief that even the best of republican politics is only for the privileged few. Moreover, contented republicanism shows no special commitment to Madisonian checks and balances. But despite any differences here, our results suggest that a revival of long-unfashionable classical republicanism might find a receptive audience.

As a reluctantly democratic discourse, B came as a surprise. However, there are many similarities between our deferential conservatism and the classical conservative tradition founded by Edmund Burke. B is very Burkean in its acceptance of the political ways that help constitute a society's tradition, its awareness of ineliminable imperfection in human nature, and in its deference to authority—hoping, perhaps, for Burke's "true natural aristocracy"? B's commitments to community, liberty guaranteed by authority and order, rather than abstract natural rights of the sort advanced by liberal philosophers, and limited expectations for governmental action are also Burkean. However, B departs from classical conservatism in its lack of interest in decentralization of power to the local level and in its unwillingness to concede that elected representatives know better than their constituents. Nor does B seem to value diversity as a protection against the leveling tendencies of mass society, so departing from twentieth-century classical conservatives such as Ortega y Gasset.

Since Burke's day, classical conservatives in the Anglo–American world have accepted the advance of democracy. Our deferential conservatism's acceptance of that advance seems at best begrudging; and in that respect, it is similar to the conservatism of the European continent. A rare American intimation of B's deferential conservatism may be found in Robert Frost's announcement, "I have given up my democratic prejudices and now willingly set the lower classes free to be completely taken care of by the upper classes"[12] (1973, 127). It would be hard to find a U.S. political theorist or public intellectual who would say as much, though hints may be found in the paleoconservative journal *Chronicles*. U.S. paleoconservatism has for the most part abandoned the local community and adopted the nation as its locus of identity, a move that resonates with B's seeming lack of interest in decentralized power within the nation-state.

The disaffected populism of discourse C has some similarity to the theory of participatory democracy, in that both envision extended, energetic, competent, and public-spirited citizenship. However, C's critique of injustice and repression in existing society is more uncompromising than many participatory theorists would allow. For example, Barber (1984) speaks mainly in terms of reforming liberal politics, in particular by taking advantage of advances in communications technology. Like most participatory democratic theorists, Barber favors extended citizen involvement in public affairs as an intrinsic good linked to personal fulfilment. In contrast, C's real motive for extended participation is the need to attack the world's injustices. Thus, C favors participation for instrumental reasons contingent upon the prevailing amount of repression. Many theorists of participatory democracy might, therefore, be happier with the attitude toward the intrinsic merits of participatory citizenship evidenced by the contented republicanism of discourse A. C's account of repression and struggle is perhaps more consistent with participatory theorists who, like Poulantzas (1980), have been influenced by Marxism and so see the need for participatory innovations that confront the state, as well as reform it (though we should note that C proclaims anticommunism). C also departs from participatory democratic theory in ascribing little importance to community and in recognizing that one should not expect too much of peoples' participatory energies.

D's private liberalism was unsurprising in that liberalism looms large in the U.S. political tradition, and liberals have always championed a private realm against excessive governmental interference. But in its thorough denigration of public life, D rejects many of the elements of Lockean and Madisonian liberalism. Liberalism has generally prized government by reasoned deliberation, especially in connection with the legislative branch. In the twentieth century, this view has been well represented by such theorists as John Dewey and Karl Popper. While, for the most part, "modern liberalism questions political authority, but typically seeks to reform it rather than merely to evade it" (Zvesper 1987, 285), D's private liberalism really does want to escape politics. D does not necessarily object to the symbols of democracy but is far more energized by the need to banish government from the private realm than by concern as to how any such government might proceed. Thus, D bears a definite resemblance to the ideas of libertarian philosophers such as Murray Rothbard and Robert Nozick. Such libertarians might, however, be uncomfortable with D's acceptance of religion as a motive force for political action, with its recognition of local community (though not of society), and with its seeming lack of concern for a free press and associated political liberties. In its selective radicalization of the liberal tradition in favor of *homo economicus,* able to calculate and pursue self-interest, D has some affinity with free market economics and the Virginia School of public choice analysis. Having discarded liberal ideas of civic virtue, D can join these schools of thought in their scorn for politics of all sorts (not just democratic politics) and in their enthusiasm for the capitalist market.

We have found no complete correspondence between any of our four discourses and any theoretical tradition. But, to cut a long story short, our results provide some encouragement to classical republicans, Burkean conservatives, participatory and populist theorists, socialists, and libertarians. The results provide less support for liberal democrats, pluralists, competitive elitists, corporatists, technocrats, and Leninists, among others.

CONCLUSION

All democratic theories—indeed, all political theories—are built upon assumptions about the capabilities and dispositions of individuals. Theories seeking transhistorical applicability claim to embody a universal paradigm of personhood, a conception of human nature. In contrast, these matters of human disposition and capability are treated here as historically and contextually contingent—as questions for empirical study, rather than definitional fiat; for reconstructive democratic theory is rooted in the way people "out there" really do think about democracy, about themselves, and about how they do or can act politically (or apolitically). Unlike its rivals, reconstructive theory is *reflexively* democratic (insofar as authentic communication is central to any model of democracy). It does not emanate from the minds of professional theorists and analysts; rather, it is developed in the context of encounters between analyst and subjects and their discourses.[13]

This connection to live possibilities in the democratic concourse guarantees that any explicitly normative analysis sensitive to our results will not be utopian or alienated from democratic practice. Anyone interested in writing normative theory can make use of our results, whether only as a point of departure for theorizing or in order to identify an audience (i.e., discourse) that might plausibly prove receptive to given ideas. Thus, theorists of participatory democracy might identify A and C as receptive audiences and B and D as formidable opposition, meriting thorough criticism. Contra Ricci (1984), we hope we have demonstrated that contributions to the "great conversation" of democratic development can involve the use of social-scientific findings. Ours is not intended as the last word in this conversation; and more discourses of democracy may await discovery, even in the single society we have studied. To shift the emphasis in our earlier adaptation of John Dewey's remark, the cure for the ailments of democratic theory is *more democratic* theory—theory that is democratic in its production, as well as its content.

Notes

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1. Dunn (1979) for example decrys the "two distinct and developed democratic theories loose in the world today—one dismally ideological and the other fairly blatantly utopian," p. 26.

2. Even self-styled iconoclasts such as Dye and Zeigler argue that the "irony of democracy" is that elites, rather than masses, are the guardians of democratic values (1987, 14–17).

3. In fairness, we should note that recent developments in survey research pertaining to cognition and information processing could conceivably revive a productive connection to democratic theory; but so far, this has not been done.

4. Later, Lane (1986) turned to survey research for further insight into individuals' conceptions of justice. But in so doing, he lost the ability to listen closely to subjects. Moreover, "the public" became a single, reified subject with just one point of view, as measured by percentages of respondents agreeing with particular statements about justice.

5. Full expositions of Q methodology may be found in Brown 1980 and 1986 and McKeown and Thomas 1988.

6. On the related use of focus groups in political inquiry, see Conover, Crewe, and Searing 1991.

7. As several subjects complained, a consequence is sexist language in a few of the statements; but it is not for us to censor statements.

8. The four factors were extracted in a varimax rotation of a centroid solution. We limited them to four because the extraction of additional factors produced, upon interpretation, a lack of clear distinction across two or more of the factors.

9. For an explicitly deconstructive use of Q, see Stainton Rogers and Stainton Rogers 1988.

10. Viability here does not necessarily mean that a theory would be easy to implement. Oppositional discourses do, of course, clash head-on with structural constraints that limit the possibilities for political innovation.

11. This possibility also supports conceptions of democracy that emphasize the discursive generation of reciprocal understanding across discourses (e.g., Barber 1984, 173–98).

12. Putnam reports the view of a British Conservative politician: "I think the weakness in a democracy really is that you get a mass of people who really take a short-term view of

what their immediate benefits are, as opposed to the person who thinks and takes a long-term view for the sake of the country" (quoted in Putnam 1973, 166). Putnam's intent relates to our own in that he seeks, in part, to identify the models of democracy held by British and Italian politicians. He develops a classification of authoritarian, polyarchal, liberal, classical, and socioeconomic models of democracy. But in our terms, his approach is not reconstructive; for he arrives at this classification by his own interpretation of answers to questions in an interview he has structured.

13. Unlike, e.g., survey research, Q methodology works best when its subjects fully understand the research questions and methodology (Dryzek 1990, 173–89; Kitzinger 1986).

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John S. Dryzek is Professor of Political Science and Jeffrey Berejikian is Doctoral Candidate of Political Science, University of Oregon, Eugene, OR 94703.

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GENERAL DETERRENCE BETWEEN ENDURING RIVALS: TESTING THREE COMPETING MODELS

PAUL HUTH *University of Michigan*
BRUCE RUSSETT *Yale University*

General deterrence, unlike immediate deterrence, has rarely been analyzed in a systematic comparative manner. We outline a research design for doing so, by studying the circumstances under which, in a set of enduring rivalries, challengers are likely to initiate militarized disputes. We indicate the conceptual and operational steps necessary to make and empirically compare predictions stemming from three often-competing theoretical frameworks: rational deterrence, a general model of rational conflict initiation, and a cognitive psychological model of behavior emphasizing risk orientation and misperception. The results of probit analysis on a pooled time series of enduring rivalries since 1945 provide support for hypotheses from each of the different theoretical models.

The most prominent international rivalry of the post–World War II era, that between the United States and Russia, has atrophied, and may evolve into a stable cooperative relationship. Yet many enduring rivalries remain, rivalries with histories extending back before World War II. Some, especially in Eastern Europe, threaten to reemerge as Russian control evaporates. Many rivalries, as in the Middle East, are characterized by competing claims to territory and periodically erupt into open threats to use military force.

Even the most severe rivalries, however, are marked by spans of time during which no overt threats are issued or violent conflict initiated. What differentiates periods of overt conflict from periods when, though the issues remain disputed and one or both parties remain dissatisfied, the threat or use of military force is not exercised? The question can be partially answered by identifying the conditions under which military–diplomatic crises escalate to full-scale war, but it equally requires understanding the conditions which lead to the emergence of crises.

Rarely do wars arise directly from peacetime conflicts of interest and nonmilitarized disputes. Rather, they generally progress through a series of escalating threats and counterthreats. Academic study has left a serious gap in understanding the entire process. On one hand, the study of crisis management and escalation is a central topic of theoretical and empirical research; so too, on the other, is the study of domestic and international conditions that produce long-term rivalries. But relatively little systematic attention has been devoted to bridging the gap—to explaining the conditions under which rivalries erupt into militarized disputes.

We shall (1) elaborate predictions derived from three different theoretical models, (2) develop a research design to test the competing predictions, and (3) present the results of probit analysis concerning the models' relative explanatory power.

CONCEPT OF GENERAL DETERRENCE

General deterrence is among the most important and least systematically studied phenomena of international politics.[1] The analysis of general deterrence differs from immediate deterrence because it focuses on conditions under which military–diplomatic crises may arise, rather than on factors that determine the outcome of a crisis once it has erupted. Inattention to the rigorous study of general deterrence is ironic, because success in immediate deterrence is only a second-best solution. Most leaders would rather prevent a crisis from emerging than pay the costs and take the risks of trying to deter an adversary once the crisis has occurred.

A standard definition of *general deterrence* describes an adversarial relationship between two states in which "leaders in at least one would consider resorting to force" to alter the status quo and "the other side, precisely because it believes the opponent would be willing to consider resort to force, maintains forces of its own and offers warnings to respond in kind to attempts to use force contrary to its interests" (Morgan 1983, 42–43). States communicate warnings by statements (ranging from officials' speeches to formal alliance commitments) defining national security interests and by the peacetime buildup and deployment of military forces. General deterrence therefore characterizes a *situation* in which the adversaries are neither using military force nor actively threatening to use it. We are not, however, implying that they are necessarily "deterred" from doing so by the military capability and will to inflict substantial costs (as in rational deterrence models). Whether such models are sufficient explanations is the object of the inquiry, not an assumption of it. A policy of general deterrence may be associated with the absence of crisis; but to identify the mere absence of crisis as a *success* of deterrence, without further evidence, prejudges the question.

By contrast to general deterrence, immediate deterrence meets three conditions: (1) officials in at least one of the adversary states are actively considering launching an attack on the other or on one of its allies; (2) leaders of the other state are aware of this; (3) the latter, to prevent an attack, issue threats to retaliate (Morgan 1983, 38). These threats and warnings are communicated by official statements or by overt military actions ranging from border reinforcements to large-scale mobilization.

A policy of general deterrence fails when the challenger couples a demand for change in the status quo with the threat or initiation of military action. This challenge may be directed initially to some other state (an ally of that adversary or even a neutral). Quite possibly, the ally's defender will respond with a counterdeterrent threat, in which case an extended immediate deterrence crisis occurs. This instance of attempted deterrence may itself succeed or fail. If, however, the "defender" issues no counterthreat, there is no crisis of extended immediate deterrence. Alternatively, the challenge may occur within a strictly bilateral context between two adversaries, in which case an instance of direct immediate deterrence has emerged.

Immediate deterrence crises often arise from failures of general deterrence. A full-blown progression from a stable situation of general deterrence to an instance when immediate deterrence fails and war ensues may encompass five stages. We simplify by suggesting a single binary choice at each stage. In reality, each stage may consist of several moves, each with multiple options:

1. One state adopts a policy of general deterrence, whereby it tries to deter another from taking steps to alter the status quo. A policy of general deterrence has been implemented.
2. A challenger makes some threat to change the status quo, a challenge that the general deterrent was intended to prevent. The policy of general deterrence has now failed.
3. The defender now decides to strengthen a commitment to its own territorial defense or to issue or enhance a commitment to an ally. The defender will have considered whether such a commitment would be worthwhile, including whether it might be provocative. The failure of a policy of general deterrence has produced a situation in which the defender adopts a policy of immediate deterrence.
4. The challenger then decides not to retreat and to press ahead despite the immediate deterrent threat of the defender.
5. Immediate deterrence has now failed, and the defender must decide whether to resist the attacker militarily.

It is important to distinguish clearly among all these stages. In previous research we established that the variables that primarily influence whether the challenger presses on or not (stage 4) are quite different from those which influence whether the defender will in fact go to war to defend its client (stage 5 (Huth and Russett 1984, 1988)).[2] If the difference in influences on decisions to fight is so great between stages 4 and 5, equally significant differences may characterize the progression from one to another of the earlier stages. It is even possible that the same variable may have opposite effects in general versus immediate deterrence situations. For instance, our unexpected finding that alliance ties between defender and protégé were negatively related to immediate extended deterrence success makes sense if the difference between general and immediate deterrence is clearly understood (Huth and Russett 1984, 1990).

The act of concluding a formal alliance would indicate a new commitment by a third-party defender to the protégé and therefore strengthen extended deterrence. (It might, however, intensify the challenger's motivation to attack preventively when it sees its long-term rival strengthened.) Furthermore, when one state is "protected" by an alliance, general deterrence will fail only if the challenger is sufficiently motivated to issue a challenge despite (or even because of) the alliance. The population of cases available for testing the success of immediate deterrence (by definition, the set of general deterrence failures) will thus have biases that differentiate it from the population of general deterrence cases. The alliance-absent cases that are included are likely to be ones wherein the challenger's motivation is weaker than in the alliance-present cases. If so, a negative relationship would emerge between immediate deterrence success and alliance, despite the positive relationship between general deterrence success and alliance.[3]

Another difference between immediate and general deterrence is the relative confidence with which success can be identified. Identification can be elusive even in situations of immediate deterrence. It is usually easy to determine if deterrence has failed (the attack occurs, or the target capitulates); but in the absence of failure it is often hard to ascertain whether deterrence has actually succeeded or whether a threatened attack was not carried out for other reasons (such as that it was never really intended). Depending on the direction in which the analyst chooses to exercise caution, the risk is of either counting spurious cases of deterrence success (false positives) or dismissing the reality of deterrence when it did indeed operate successfully (false negatives). The classic distinction between the two types of error applies, and there is no perfect rule for steering between them (Huth and Russett 1990).

In general deterrence, the false-positive-versus-false-negative problem is magnified. Perhaps that is one reason why few systematic studies exist. Failures of general deterrence are by definition obvious when a crisis erupts. But general deterrence success is, in Sherlock Holmes's terms, that of a dog that didn't bark in the night. It is hard to distinguish challenges that are truly deterred from those in which the reputed "challenger" in fact had no intention of mounting a challenge anyway or those for which the adversary's military capabilities and resolve to use them, as emphasized by the rational deterrence model, were essentially irrelevant. At least in immediate deterrence we have some overt diplomatic demands and military threat to suggest, perhaps imperfectly, the intention to challenge. But in the study of general deterrence, there is an especially serious risk of inflating any "sample" of general deterrence cases with false positives and often the possibility of infinite dispute over decision makers' intentions (e.g., over Soviet intentions vis-à-vis Western Europe after 1945). Alternatively, an analysis only of instances of general deterrence failure removes all variance from the dependent variable, as well as introducing unknown selection bias into the sample.

We cannot fully resolve this dilemma, but we utilize a research design and data base that helps to determine whether the absence of conflict should be equated with a possible deterrence success. We look at protracted periods of rivalry between states, punctuated by crisis and war, under changing international and domestic conditions. Even so, we *cannot be certain* that every year without an overt challenge represents a deterrence success, since the absence of challenge may derive more from domestic political conditions than from the deterrer's policies.

ENDURING RIVALRIES

The population of enduring rivalries in the international system includes all dyadic relations in which a dispute created the possibility of one or both parties resorting to overt military force to achieve a gain or redress a grievance. We would like, in principle, to talk about all enduring rivalries; but since that is too large a population to analyze here, we delimit a subpopulation. First, we limit ourselves to those post–World War II disputes in which competing claims to territory are publicly expressed by governments. This decision provides some constancy in the international system (postcolonial, nuclear era) and includes the period during which data are most readily available and reliable.

Second, we focus on cases of direct, rather than extended, deterrence. Different (though related) models need to be constructed for careful testing of each. In situations of extended deterrence, the credibility of the defender's deterrent threat is much more likely to be questioned by the challenger; and this important difference must be incorporated into analytical models. So we limit our subpopulation to disputes over competing claims to national (not colonial) territory or claims in which one party rejects the other's claims to sovereignty. In these disputes, one or both states actively seeks to alter the territorial status quo at various points in time. Although not all enduring rivalries involve disputes over territory or sovereignty, in the postwar period, most rivalries that have erupted into military conflict have concerned competing claims to territory and the population and/or resources within it.

TABLE 1

Enduring Interstate Rivalries, 1948–82

CHALLENGER	TARGET	YEARS	NUMBER OF DISPUTE INITIATIONS
Argentina	Chile	1948–82	10
Chile	Argentina	1948–77	4
China	India	1950–62	4
India	China	1961–82	3
China	USSR	1963–82	6
Ecuador	Peru	1949–82	5
Egypt	Israel	1948–79	8
Greece	Turkey	1960–82	3
Turkey	Greece	1974–82	2
Iran	Iraq	1948–75	9
Iraq	Iran	1976–82	2
Pakistan	India	1948–82	13
Somalia	Ethiopia	1960–82	5
Syria	Israel	1948–82	19

Note: One or both parties may be characterized as the challenger—and hence the object of deterrence—in our analysis. We begin the analysis with 1948 due to limited data availability even though territorial claims may antedate that year. Israel–Jordan has been omitted; deterrence is largely contingent on the success of Israeli deterrence of Egypt and Syria and therefore the dyad would not be statistically independent. There are some less severe cases of nonindependence (the two involving India and the two involving Israel); but in our theoretical model, we do include variables for the dispute involvement of challenger and target with other cases.

This decision excludes some great-power rivalries (e.g., the one between the United States and the USSR) that are better understood as primarily concerning extended deterrence to protect allies, rather than deterrence of an attack on themselves. Furthermore, we exclude territorial disputes between smaller states in which long-term military deployments by a great power on the territory of one or both rivals was directed against the other power. Such situations (e.g., North and South Korea) are best understood as cases not of direct deterrence but of extended deterrence by the great powers.

Third, the rivalry must be enduring (20 years or more) and be marked by at least five international disputes initiated by the challenger or challengers over territory characterized by threat or use of military force by central authorities.[4] This ensures that general deterrence will indeed "fail" often enough to provide sufficient variance in the dependent variable for systematic comparative analysis.[5]

Table 1 shows the rivalries which meet these conditions.[6] We begin a rivalry with the first publicly expressed claim of one government to the territory of the other and end it only when a formal agreement resolving competing claims is implemented that is, when both parties accept it.

Our coding rules focus on cases with at least periodic military threats or actions that leave behavioral traces. Logically, the concept of general deterrence is appropriate: (1) one party (or both) is not satisfied with the status quo, so there is a possible challenge to be deterred; (2) the military option is sufficiently viable for the dissatisfied state or states that military action was sometimes actually taken or threatened; and (3) given the history of periodic manifest threats, each party is likely to view the other as a primary security threat. Most of the severe crises and wars in the post–World War II era have been between states entangled in enduring rivalries. Thus, we are studying general deterrence where it is important, and at the same time relatively difficult, to ensure.

We may be missing years before any overt territorial claim is issued, in which general deterrence operated to prevent such a claim from being made. By omitting nonterritorially based disputes, we may be missing other instances of direct general deterrence. Yet even severe ethnic or ideologically based conflicts are less likely to erupt into militarized disputes when competing territorial claims are not at issue. Including either years before a territorial claim was made or nonterritorially based disputes would risk introducing a bias toward imputation of general deterrence where it was not relevant. For similar reasons we omit rivalries in which dissatisfied states made fewer than five overt threats or actual uses of force. Again, we prefer missing some cases of successful general deterrence to the risk of inferring that military deterrence was effective when it may not in fact have been needed or relevant.

Our selection criteria, deliberately chosen to minimize the number of false positives, mean that we will not have an accurate estimate of *how frequently* general deterrence succeeds or fails in broader international relations. But our conclusions about *why* general deterrence succeeds or fails will lack wider validity only if success or failure is related systematically to our selection criteria. The most likely source of bias would be that rivalries marked by few overt militarized disputes may more frequently be characterized by a severe military imbalance against the challenger, inhibiting challenge as a viable policy option. If so, our results, if generalized to those rivalries, would underestimate the relative effectiveness of a military balance favoring the defender. As a result, it will be difficult to generalize confidently beyond the rivalries in Table 1; but we consider any such generalization much more soundly based than some of the familiar efforts to generalize from immediate deterrence cases to general deterrence ones.

THEORETICAL FRAMEWORK

General deterrence provides the overarching framework within which we analyze enduring rivalries. A deterrent relationship may be affected by variables at three levels of analysis:

1. *Dyadic Relationships*. Here we include relative power balances and shifts in military capabilities stemming from, for example, arms races or power transitions, as well as the broader diplomatic state of relations as reflected in the presence or absence of progress in negotiations over disputed territory.
2. *Relations of Both Rivals with Other Powers in the Wider International System*. We focus on the present and previous status of each adversary's militarized disputes with other states.
3. *The Domestic Political Environment in Which Foreign Policy Elites Operate*. How secure do incumbent leaders feel from challenge? How great is their fear of loss of governing authority? This depends on the strength of domestic political challengers and manifestations of political protest or insurgency.

We will now outline three different theoretical models: rational deterrence theory, a more general rational choice model of conflict initiation, and a cognitive psychological decision-making model. Each emphasizes different levels and incorporates variables in different ways. The *rational deterrence* model operates at the first level, when it concentrates on variables in the challenger's calculus which the deterrer can manipulate, and also at the second. The more *general* model of conflict initiation also considers these two levels but additionally pays attention to domestic factors affecting a rational leader's hold on power. The *cognitive psychological* model typically pays greatest attention to the internal influences but does not employ the assumption of rationality. Rather, it emphasizes the likelihood that under conditions of a deteriorating domestic or international power position decision makers will be more likely to take risks and/or to misinterpret their adversaries' capabilities and intentions, especially in the form of motivated misperception. An adequate test of these competing models must therefore include relevant variables from all three levels and (in the case of domestic political variables) provide a means for distinguishing between the rational and cognitive psychological perspectives.

The Rational Deterrence Model: Relative Power

We now elaborate each model and our operational measures. The primary hypothesis of the rational deterrence model[7] is that a challenger will be more likely to initiate a militarized dispute, the lower the expected costs of armed conflict.[8] In making evaluations, the challenger will consider the short-term dyadic military balance. Challengers typically consider their ability to win a relatively quick war:[9] victory in protracted war is unattractive due to the economic and military losses and political risks entailed. As a result, the challenger's estimate of the probability and cost of victory will be based on the military capabilities each side can bring to bear in the initial months of war—the relative size of standing military forces and preexisting stockpiles of weapons—and not the longer-term mobilization base of overall population and industrial capability.[10] To measure the short-term balance of forces, we combine the balance of total manpower on active duty, the balance of military expenditures, and the balance of expenditures per soldier.

We also will test for the effect of *changes* in relative power by including variables that measure the magnitude of an arms buildup, as well as whether a power transition is occurring. For both, we hypothesize a positive relationship to dispute initiation by the challenger. Within the theoretical literature on arms races, a longstanding hypothesis is that increasing levels of a mutual buildup in military capabilities between adversaries not only reflect, but also contribute to, an escalating spiral of hostility, suspicion, and deterioration in political relations (Siverson and Diehl 1989). Conflict therefore becomes more likely as the willingness of either side to make concessions diminishes. For the challenger, the diplomatic option becomes less attractive as means to change the status quo and the threat or use of force become more attractive. The arms buildup variable is measured as the product of the rate of growth in military expenditures for both challenger and target in the previous year.

In the post-World War II period, the military capabilities of Third World states and regional powers have been significantly influenced by the quantity and quality of arms imported from the major powers. As a result, we have also included, as a separate measure of change in relative capabilities, the level of arms imported by each rival over time. We hypothesize that the greater the level of arms imported by the challenger, the more likely the challenger is to initiate a militarized dispute. Conversely, increasing imports of arms by the target should decrease the likelihood that the challenger will initiate a militarized dispute. We measure arms imports as the *z*-score lagged one year for the constant dollar value of major weapon systems imported annually by challenger and target.[11]

While the concept of a power transition has traditionally been applied to the outbreak of major wars between Great Powers (Doran 1989; Kugler and Organski 1989), the concept can also be applied to relations between rival states at the regional level. A power transition increases the probability that a challenger will initiate a dispute for two reasons: (1) if the challenger's current position of military advantage is diminishing, the challenger has an added incentive to confront its adversary before its relative position shifts to one of disadvantage (Levy 1987); and (2) conversely, if the challenger's military position is improving relative to its adversary, the challenger may become more assertive in its demands for a change in the status quo and more confident in its ability to force the adversary to make concessions. The power transition variable is measured as a dummy variable coded 1 if (1) the trend in the growth of the military balance for the challenger is either less than $-.1$ or greater than $.1$ and (2) the ratio of the challenger's military capabilities to target's is between .75 and 1.33.

A more complete test of a rational deterrence model must also consider the recent international conflict behavior of challenger and adversary. If the challenger is currently in a militarized dispute or war with another state or in the previous year was involved in a war that it lost or fought to a stalemate, we would expect it to be less likely to initiate a dispute against its rival, because it will be preoccupied or recovering. On the other hand, if the rival is involved in another dispute or was stalemated or suffered a defeat in a war in the previous year, the challenger would be more likely to initiate a militarized dispute against the rival in expectation that the adversary could be bullied or defeated. If any of these conditions of dispute or war apply to the challenger or adversary we code a 1 for the challenger's or adversary's side, as appropriate.

The General Rational Model: Domestic Politics

The general rational model of conflict initiation includes these variables regarding military capability and will of the adversary but, in addition, considers the *costs of inaction* for the challenger.[12] We conceptualize the costs of inaction in terms of the domestic and international political conditions of the challenger. *Domestically*, the incentives to initiate international conflict include those identified in the large corpus of social-psychological theories asserting that leaders of groups may often try to divert hostility derived from frustrations into aggressive words or acts toward outsiders (Levine and Campbell 1972, chap. 8; Levy 1988, 1989; Shaw and Wong 1988). While earlier work has not found consistent evidence for the causal connections between internal and external conflict (for reviews, see Stohl 1980; Zinnes 1980), the reason may be the failure to specify properly a complex theoretical relationship.

The stronger the coefficients for variables measuring the state of domestic political conditions relative to the dyadic power balance variables, the more plausible will be the argument that the eruption of conflict represents something more than a failure of military deterrence. We begin with the assumption that decisions to threaten the use of force internationally are considered by a small number of high-level political and military leaders, with final approval required by the recognized leader of that group. That assumption focuses on the need of the leader and his principal advisers to be concerned not only with the state's international position but also with their own position within the state's political system. For example, in democracies, leaders' political popularity depends heavily on the condition of the national economy. Nevertheless, some politically relevant goals may be incompatible (e.g., increase employment and reduce inflation); and the instruments for manipulating the economy are crude and unreliable (a democratic leader who wishes to garner electoral support by stimulating the economy may be unable to do so). From the perspective of a rational, self-interest-maximizing leader, an alternative to manipulating the economy may be to divert dissatisfaction or antagonism toward a longstanding rival. There is increasing evidence of this behavior when the economy is weak, when elections are near, and (especially) when poor economic conditions and elections coincide.[13]

The situation is much less clear-cut for nondemocratic states. Authoritarian leaders may not have such strong incentives to externalize domestic discontent. The immediate political pressures of national elections are unimportant or absent. An authoritarian leader with some reasonable claim to political legitimacy or with an effective internal security apparatus to repress dissent may feel little need to provoke or intensify an international quarrel. But regimes that lack popular legitimacy and strong internal security forces may have incentives to externalize hostility so as to avert coups against them. In our data set on enduring rivalries, there are few years of democratic government, and even fewer of democratic elections. Thus, we do not test the effects of economic conditions but focus, instead, on direct manifestations of domestic political discontent.[14]

We first consider the effects of tensions and dispute among high-level government elites. This is reflected in the reshuffling of cabinet appointments and the resignation or dismissal of high officials over policy matters. In such situations, the political leadership is reacting to rising dissatisfaction by taking steps to cut off dissent within the government and/or to convey to the public that new initiatives will be taken. We hypothesize that under such conditions the political leadership is more likely to adopt an aggressive foreign policy in the expectation that rally-'round-the-flag effects will help to stymie elites who may have been considering a coup.

However, once levels of overt protest and rebellion begin to rise above a very low level, the leadership may not be as quick to externalize—with the dampening effects strongest for increasing levels of rebellion. High levels of protest—and even moderate levels of rebellion—represent a threat to all regimes; as a result, decision makers are confronted with cross-cutting pressures to externalize. On the one hand, estimates of the probability of victory in armed conflict are likely to be reduced for the challenger, since its armed forces may already be committed to suppressing internal unrest or may have to be held in reserve for such possibilities. On the other hand, the marginal value of rally-'round-the-flag effects is greater, given the low level of support. Thus, decision makers in the challenger state are likely to value very highly the possibility of a military or diplomatic victory over their longstanding rival. The net effect is indeterminate. The two components may cancel one another, in which case there would be no consistent relationship between levels of protest and rebellion and dispute initiation by the challenger. Conversely, if decision makers weight the expected benefits of dispute initiation more heavily than the reduced chances of success, then a positive relationship would exist between protest/rebellion and initiation. A negative relationship would be present if decision makers weighted the utilities and probabilities in the opposite way.

Data on political conditions within the challenger state are from the daily domestic events file of the Conflict and Peace Data Bank 1948–78 and the *World Handbook of Political and Social Indicators* (Taylor and Jodice 1983). Both data sets are deposited at the Inter-university Consortium for Political and Social Research. Intragovernmental and elite conflict are operationalized as the z-score for the annual number of days in which one or more of the following events occurred: policy disputes between government agencies or officials, change in cabinet appointments, or resignation/dismissal of major government officials. Political protest is operationalized as summing the z-scores for the annual levels of strikes and demonstrations and lagged one year. Rebellion is measured with two related indicators: (1) the z-score for the annual number of days in which acts of political violence (actual or attempted coups, large-scale riots, strikes accompanied by violence, and mass arrests) were committed by government or nongovernmental groups; (2) the z-score for the annual number of armed attacks and deaths from domestic political violence, lagged one year.[15]

Leaders of the challenging state are likely also to consider whether domestic conditions within their *adversaries* will make their adversaries more or less capable of responding to a diplomatic/military challenge. Since we are applying the general rational actor model to the challenger, we will here assume that leaders of the challenger in turn expect their adversary to respond rationally. The general rational choice model predicted that as intragovernmental tensions increased, political leaders would have greater domestic political incentives to initiate conflict. Because the adversary government will be unlikely to back down in a confrontation, the challenger's incentives to initiate a militarized dispute should be reduced.

Seeing relatively *high* levels of protest or rebellion in the adversary, however, the challenger will have to consider how the contradictory pressures we have discussed will ultimately shape the decisions of the adversary. Once again, the net effect is indeterminate. If the challenger believes that the adversary's armed forces will have to be diverted to maintaining internal order, then it may view the situation as a favorable opportunity to initiate a militarized dispute, calculating that the adversary either can be forced to make concessions under coercive pressure or can be militarily defeated without a protracted armed conflict. However, if the challenger reasons that the adversary has a great deal to gain in terms of increased domestic support by a diplomatic or military victory and will therefore take whatever military actions are necessary to achieve such a victory (even at the risk of a further breakdown of internal order), then it may be less likely to initiate a dispute. Variables measuring levels of intragovernmental tensions, political protest, and rebellion within the target are operationalized in the same manner as for the challenger.

We hypothesize that the utility the challenger attaches to the prevailing territorial status quo is also a function of whether the challenger believes that negotiations and diplomacy are viable alternative options to the threat or use of force. Leaders within the challenger may regard the military option as too risky even under favorable conditions and therefore to be turned to only if the diplomatic option fails to alter the status quo at less risk and cost. Foreign policy elites within the challenger can then cite the failure of diplomacy as justification for resorting to the military option as a means to change the status quo. A failure of negotiations to achieve even limited gains can create domestic discontent and thereby create pressures within the foreign policy leadership of the challenger to resolve the dispute forcefully. Specifically, the likelihood of dispute initiation by the challenger decreases if the challenger and target are actively engaged in negotiations over the disputed territory during that year. However, the probability of dispute initiation increases if negotiations in the previous year failed to achieve favorable changes in the territorial status quo for the challenger. Identifying periods of negotiations and the outcomes of such talks was based on reading a wide range of secondary sources summarizing and analyzing relations between challenger and target.[16]

Cognitive Psychological Models: Risk and Misperception

These models depart from the assumption of rationality by hypothesizing that (1) the risk attitude of decision makers varies with the situational context they face and (2) under certain conditions, decision makers will be motivated to misperceive the military capabilities and will of their adversaries.[17]

Decision makers' orientations toward risk condition their response to uncertainty about the outcome of armed conflict. We define *risk orientation* generally as an individual's way of choosing between options that have the same expected payoff but vary with respect to the probability of receiving that payoff. Risk-averse actors tend to favor more certain outcomes, while risk-acceptant actors are more willing to gamble on less certain outcomes (Arrow 1982). Given the same level of confidence in the estimate of expected costs, risk-acceptant and risk-averse actors will respond differently. Specifically, our conception of risk attitude draws on the experimental findings of cognitive psychologists, who argue that risk attitudes vary systematically with the situational context in which decisions are made (Kahneman and Tversky 1979; Quattrone and Tversky 1988). The situational context, in turn, is defined in terms of either the domain of losses or the domain of gains. When decision makers frame their choice from the perspective of trying to avoid losses, they are likely to be risk-acceptant. Conversely, when the choice among options is viewed as an opportunity to make gains, then decision makers will be risk-averse. This conception of risk attitude as contextually determined differs from the individually oriented concept common in international relations research, whether in many rational decision-making models (e.g., Bueno de Mesquita 1981, 1985) or in the psychological tradition emphasizing personality differences at the individual level.

Leaders of states have a primary goal of retaining power. Under "normal" conditions of political stability, they are in the domain of gains and so are averse to taking risks in the international arena. But if their hold on power becomes threatened by domestic instability, they are in the domain of losses, wherein they become willing to accept risks internationally so as to externalize domestic discontent. We further argue that risk propensity interacts with a measure of *relative power (uncertainty)*. When facing losses due to a weak domestic power position, the challenger will be more willing to gamble with the risks of international conflict if the international power balance is uncertain. In the absence of a deteriorating domestic position, uncertainty will enhance caution (risk aversion) and restraint toward initiating international conflict.[18] Thus, a challenger in a risk-acceptant context will be more likely to initiate a militarized dispute, the greater the uncertainty in estimating expected costs. Conversely, a challenger in a risk-averse context will be less likely to initiate a militarized dispute, the greater the level of uncertainty.

We code the challenger as being in the domain of losses if the z-scores for either intragovernmental tensions, protest, or rebellion were greater than zero. Each individual z-score is then combined into a separate interactive variable with a measure of uncertainty. The measure of uncertainty is operationalized as the previously discussed power balance if the ratio is less than or equal to one and the inverse of the power balance if the balance is greater than one. (Certainty increases as the military balance moves away from one in either direction.)

The version of the cognitive psychological model stressing motivated misperception generates predictions that differ from both the rational deterrence model and the general rational model. According to the rational deterrence model, a challenger will be deterred by an unfavorable military balance. The general rational model, incorporating domestic politics, predicts that the initiation of militarized disputes will be more common under conditions of intragovernmental tensions but indeterminate under conditions of protest and rebellion within the challenger.

The *motivated misperception* model concurs with the general rational model in predicting a positive relationship between intragovernmental tensions and externalization. But the motivated misperception model disagrees with the rational deterrence model by arguing that an unfavorable balance of forces will not deter a challenger. Furthermore, it disagrees with the general rational model by arguing that high levels of protest or rebellion will consistently generate incentives to externalize.[19] Wishful thinking may encourage leaders to believe that an external adversary will back down if confronted and that the people at home will rally despite their discontent.[20] By this reasoning, the motivated misperception model predicts that under conditions of high levels of protest or rebellion, the challenger is likely to initiate a militarized dispute even if the power balance is unfavorable; that is, an interactive variable for an unfavorable power balance *times* high domestic instability will show a statistically significant positive coefficient. In other words, if domestic instability is high, an unfavorable power balance will not exert its customary deterring effect.

To test this hypothesis, we created three different interactive measures for the misperception variable. The first measure combined an unfavorable military balance (the ratio of challenger to target military capabilities was less than one) with the *z*-score for protest when it was greater than zero. The other two measures interacted an unfavorable military balance with the two different indicators of rebellion in cases where the *z*-score was greater than zero.

Finally, a rational challenger may consider the implications of motivated misperception affecting the behavior of the adversary. In crisis, adversaries typically view each other as substantially rational actors (Holsti 1972). The assumption of an adversary's rationality enables decision makers to ignore or discount the opponent's capacities for misperception (Jervis 1976). A challenger who expects the adversary also to behave rationally will predict that an adversary preoccupied with severe internal-order problems may be unable to respond effectively. This, then, may seem a safe opportunity to achieve major gains at the adversary's expense. But if the challenger allows for the possibility of motivated misperception, the opportunity may not seem so promising. If the challenger seeks to achieve political concessions without resort to force, wishful thinking by the adversary may lead it to fight, rather than to concede, even though a rational evaluation of the balance of forces would dictate concession.[21] This theoretically interesting distinction cannot be tested, however, without making a priori judgments about the challenger's goals.

DATA ANALYSIS

We ran a probit analysis on a set of pooled time-series data to estimate how change in variables within a dyad relates to differences in the timing of disputes within that dyad. Table 2 shows the results.[22] They support elements of all three models tested: relative power balances, domestic political conditions, and motivated bias all have a significant impact on decisions by the challenger to initiate militarized disputes. Each model by itself, however, provides only a partial explanation. The most satisfactory explanation requires a synthesis of how domestic political and international power conditions affect the calculations of a challenger from the perspectives of rational choice and of cognitive psychological models of decision making.

TABLE 2

Probit Results for Dispute Initiation (N = 278)

VARIABLE	COEFFICIENT	STANDARD ERROR	SIGNIFICANCE LEVEL
Constant	−1.13	.18	.001
Deterrence Model			
Military balance	.11	.07	.06
Arms race	4.47	1.73	.005
Power transition	.59	.33	.04
Arms imports (C)	.12	.11	—
Arms imports (T)	−.07	.13	—
Disputes (C)	−.45	.24	.04
Disputes (T)	.61	.20	.001
General Rational Choice Model			
Elite conflict (C)	.23	.11	.025
Protest (C)	−.16	.22	—
Rebellion I (C)	−.13	.16	—
Rebellion II (C)	−.42	.14	.005*
Elite conflict (T)	.12	.11	—
Protest (T)	−.28	.12	.025*
Rebellion I (T)	−.09	.10	—
Rebellion II (T)	−.23	.13	.09*
Negotiations	−.21	.22	—
Stalemate	.22	.21	—
Cognitive Psychology Model			
Misperception	.38	.20	.025
Risk propensity	.39	.38	—

*Two-tailed test.

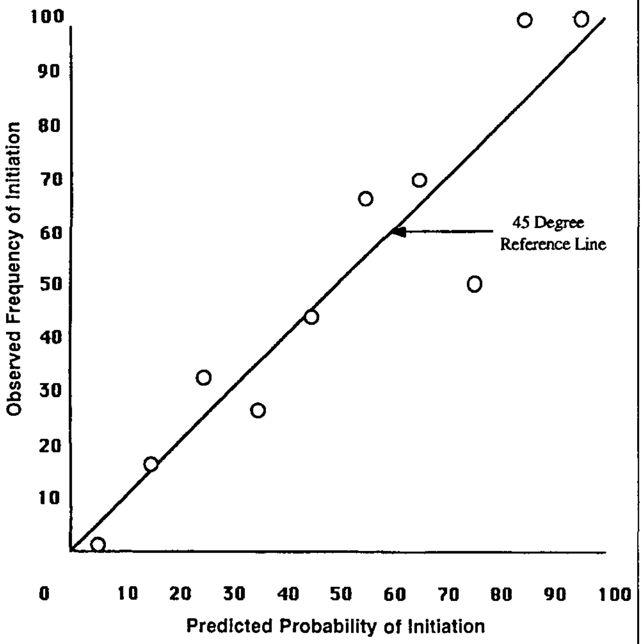
Note: C = challenger; T = target. Rebellion I is measured as *z*-score for the annual number of deaths due to political violence lagged one year; rebellion II is measured as the *z*-score for annual number of days of violent political behavior. The misperception variable is measured as the rebellion I score of the challenger when it is above 0 for the cases where the ratio of challenger to target military capabilities is less than 1.0.

The overall ability of our model to forecast the likelihood of dispute initiation is presented in Figure 1, a calibration table often used in analyses of decision-making accuracy (Yates 1990). It summarizes the correspondence between the predicted probabilities that a target event will occur and the actual frequencies with which the event takes place. Perfect calibration would be represented by a 45-degree upward sloping diagonal line from the origin and a calibration index score of 0. Conversely, the worst possible level of calibration would be represented by a downward sloping diagonal line and a calibration index score of 1.[23] Our calibration line's slope is slightly greater than 45 degrees, reflecting the fact that our model tends to overpredict the initiation of disputes. If one takes the parameter estimates in Table 2 and sets the variables to their mean values, our model predicts a 44% probability of initiation. In our data set, initiations actually comprise 28% of the cases. Nonetheless, the fit between our forecast probabilities and the actual frequency of disputes is quite good; and the calibration index is only .0049.

In Table 2, all of the coefficients for the variables in the deterrence model are in the predicted direction;

FIGURE 1

Calibration Table of Probit Model



Note: The small circles in the figure represent clusters of cases that fall within each decile of the predicted probability of initiation. A perfect fit between the predicted probability and the observed probability of initiation is represented by the 45-degree reference line (i.e., a slope of 1 and an intercept of 0) and a calibration index score of 0. The actual fit of the model reported in Table 2 is represented by a line with a slope of 1.03, an intercept of −.01, and a calibration index score of .0049.

TABLE 3

Marginal Impact of Selected Variable on the Probability of Dispute Initiation

CHANGE IN VALUES OF INDEPENDENT VARIABLE	CHANGE IN PROBABILITY OF DISPUTE INITIATION (%)
Deterrence model	
Ratio of forces	
0.5 to 1.0	+2.2
1.0 to 2.0	+4.4
Growth rate in military expenditures	
Min. −3.5 to mean .14	+40.6
Mean .14 to Max. .33	+45.4
Power transition, *no* to *yes*	+21.2
Challenger involved in another militarized dispute, *no* to *yes*	−17.6
Target involved in another militarized dispute, *no* to *yes*	+21.8
General rational choice model	
Elite conflict (challenger)	
Min. −.41 to mean 0	+12.1
Mean 0 to max. 3.77	+31.8
Rebellion (challenger)	
Min. −1.1 to mean 0	−18.2
Mean 0 to max. 4.9	−43.1
Protest (target)	
Min. −1.19 to mean 0	−12.1
Mean 0 to max. 4.63	−37.3
Rebellion (target)	
Min. −.97 to mean 0	−8.7
Mean 0 to max. 4.98	−34.2
Cognitive psychological model	
S.D. above av. lev. intern. unrest when bal. of forces is unfav.	
.0 to .5	+5.0
.5 to 1.0	+6.0
1.0 to 2.5	+19.0
2.5 to 5.0	+28.0

Note: The changes in the probability of dispute initiation are calculated from the coefficients in the equation in Table 2 by changing the value of a single variable while holding all other variables in the model at their mean or modal values. The change in position on the cumulative standard normal distribution is then converted into the percentage change in the probability of a deterrence success.

and five of seven are statistically significant. Thus, the deterrence model does well despite some selection and measurement problems that might (as we noted) be expected to attenuate the empirical relationship. When we examine the marginal impact[24] of the significant variables in the deterrence model (see Table 3), the substantive impact of the *static* balance of military capabilities is not large. For example, the difference between a two-to-one advantage for the target to a two-to-one advantage for the challenger increases the probability of dispute initiation by only about 7%. Variables measuring *change* in the military balance—arms races and power transitions—have a larger impact, however. The probability of dispute initiation increases by about 45% if you compare a situation of no growth in military expenditures to a case in which the combined growth of rival and challenger is 33%. The presence of a power transition between challenger and target increases the probability of dispute initiation by 21%. Finally, the current dispute involvement of challenger and rival also plays an important role. If the challenger is engaged in a militarized dispute with another state, the probability of initiating against the rival decreases by almost 18%. Conversely, if the rival is embroiled in a dispute with another state, the challenger is about 22% more likely to initiate a conflict with the rival.

The general rational model produces findings that are more mixed than those for the deterrence model,[25] but the substantive interpretation points to an important general conclusion: domestic turmoil, once it becomes mass-based or includes violence, does not act as a spark to international conflict but, instead, induces restraint by foreign policy leaders. There are exceptions, however, and we will discuss them when we consider our results concerning motivated misperception.

The coefficients for elite conflict, protest, and rebellion within challenger and target show that four out of the eight variables are statistically significant at the .10 level or higher. A curvilinear relationship emerges when we compare the parameter estimates across the various measures. As elite-level conflict within the government increases, the challenger becomes more likely to initiate a dispute. For example, in the middle of Table 3, the movement from a low to high level of *elite conflict* generates a *greater-than-40% increase* in the probability of dispute initiation. In contrast, as political conflict becomes mass-based and visible (protest) without turning to violence, leaders within the challenger are no longer more likely to initiate a dispute—in effect, there is no systematic impact if we look at the coefficient in Table 2. However, when mass political behavior becomes increasingly violent (rebellion II, the *z*-score for annual number of days of violent political behavior), then leaders within the challenger are less likely to initiate a dispute. In the middle of Table 3, the movement from low to high levels of *rebellion* produces a *greater-than-60% decrease* in the likelihood of dispute initiation.

The challenger's reaction to political instability within the target state is complex and indicates that turmoil within a state does not generally make it a more likely target of coercive pressure. In fact, the opposite pattern is more prevalent. For example, as conflict within the government and ruling elite of the target increases, there is only weak evidence ($b = .12$, with t-ratio only 1.08) that the challenger becomes more likely to initiate a dispute. However, as protest and rebellion increase in the target, the challenger becomes less likely to initiate a dispute (see middle of Table 3). For example, as protest and rebellion move from low to high levels within the target, the probability of dispute initiation by the challenger decreases by over 40%. A plausible inference is that decision makers within the challenger recognize that elites in the target have very strong incentives to stand firm, since a diplomatic or military victory may represent a great opportunity to restore their authority and political standing.

The coefficients for the final two variables in the general rational choice model—the presence of negotiations and their outcome—have the predicted signs but are not statistically significant.

The final two variables listed in Table 2 show some support for the cognitive psychological model. The coefficient for risk propensity in interaction with uncertainty is positive, as hypothesized, but not statistically significant.[26] The motivated misperception hypothesis receives stronger support from the positive and significant coefficient for this variable in Table 2. Under conditions of high rebellion (lagged *z*-score for deaths due to political violence) within the challenger and a balance of military forces favoring the target (the ratio is less than 1.0), the probability of dispute initiation by the challenger increases.[27] Finally, as presented at the bottom of Table 3, when rebellion within the challenger increases from a low to a very high level despite an unfavorable military balance, the probability of dispute initiation increases by over 50%. This partially cancels the effect (from the general rational choice model) of rebellion in discouraging the challenger from initiating a dispute. The combined effect of high levels of rebellion and an unfavorable military balance is explosive.

CONCLUSION

General deterrence, unlike immediate deterrence, has rarely been analyzed in a systematic comparative manner. Here we do so, studying the circumstances under which, in a set of enduring rivalries, challengers are likely to initiate militarized disputes. We have taken the conceptual and operational steps necessary to compare predictions stemming from three often-competing theoretical frameworks: rational deterrence, a general model of rational conflict initiation, and a model of cognitive behavior emphasizing risk orientation and misperception. The results of a pooled time-series probit analysis indicate that each model includes important elements of truth. Parsimonious models of conflict initiation within a general deterrent relationship, utilizing either a rational choice model alone or a cognitive psychological model alone, are inadequate. Further probit analysis needs to be done on the individual rivalries, as does intensive nonstatistical historical analysis of cases. The goal should not be merely to demonstrate which model has superior explanatory power but to combine and integrate them.

Notes

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1. See, however, George and Smoke 1974; Lebow 1981; Wallace 1982; and Weede 1983.

2. This empirically observable difference follows from theoretical considerations about the role of immediate and short-term balances in an attacker's decision to try for a fait accompli, contrasted with the defender's focus on whether it is willing to fight a war of attrition, since the immediate balance presumably is not favorable. Alliance commitments also cut different ways in these two stages. The attacker may not appreciate that the failure of the defender to honor its alliance will have long-term reputational consequences with which the defender may be vitally concerned.

3. We have elsewhere suggested this interpretation post hoc (Huth and Russett 1984, 517); and Fearon (1990) offers a deductive argument formalizing this and other initially counterintuitive possibilities.

4. A complete tentative listing of militarized disputes within postwar enduring rivalries is presented in Diehl and Goertz 1991. The use or threat of military force is taken from Gochman and Maoz 1984. It excludes actions taken by local troops or commanders without central authorization but includes actions by forces of one government as part of "rebel" forces in the adversary state.

5. Even if one questions our argument that direct and extended deterrence cases should be analyzed separately, extended deterrence cases involving North and South Korea or China and Taiwan would be excluded because fewer than five militarized disputes can be identified. Other direct deterrence rivalries (e.g., Iraq and Kuwait, Libya and Chad) also have too few disputes.

6. This is a tentative list based largely on the Correlates of War (COW) Militarized Dispute Data Set which is currently under revision. The list in Table 1 reflects our best efforts to identify enduring rivals, given the state of the data base at this time.

7. There is, of course, a very large literature on rational deterrence theory. Some of the most important early works include Brodie 1959; Ellsberg 1960; Kahn 1960; Kaufmann 1956; Schelling 1960, 1966; and Snyder 1961.

8. The challenger will then compare these expected costs to expected benefits and decide whether to threaten or use force. In our model, we do not include a variable measuring the value of the territory in dispute (an indicator of expected benefits if the territorial status quo is overturned) because our objective is to explain when disputes will emerge over time within an enduring rivalry. We assume that within an enduring rivalry both parties place a high value on the territorial issues in dispute and therefore the value of the territory will be constant. In this model, the expected outcome of conflict is a function of military capabilities *times* the probability that those capabilities will be employed. In direct deterrence, a challenger may reasonably assume that the adversary will respond militarily to an attack (unlike extended deterrence, where the challenger must estimate the *probability* that the "defender" will fight). Thus, here, the challenger's estimate of costs is a direct function of relative capabilities. A further complication involves the challenger's estimate of the adversary's ability to employ its military capabilities *effectively*; this, in turn, depends in part on the adversary's internal political conditions, which we shall address.

9. A number of factors help to determine the military balance, including size and quality of troops and weapons, geography, and the interaction of opposing strategies. The literature on assessing the conventional balance of forces between NATO and the Warsaw Pact was built on the assumption that the military balance is the most important variable in determining the prospects for deterrence along the Central Front. See, e.g., Epstein 1989; Mako 1983; Mearsheimer 1983; and Posen 1984/85.

10. Our and Mearsheimer's previous empirical work on immediate deterrence (Huth 1988; Huth and Russett 1984; Mearsheimer 1983) strongly supports this argument. Variables measuring the local and short-term balance of forces had a significant impact on deterrence outcomes, whereas a variable measuring the long-term balance of forces, including population size and industrial capacity, did not have a significant effect. Ideally, relative military power would be measured in terms of elite perceptions; but such data are not available. Instead, we use objective indicators of military power from the COW National Capabilities Data Set. We assume that perceptions of military power are sufficiently correlated with objective indicators to permit us to use them in our empirical analysis. In addition, the measures of a rival's military power may be inflated by the need to deter a third party or to suppress domestic insurgency: if so, the empirical measure will not represent a fully accurate indicator of threat or deterrent capability toward the other rival, and the empirical relationship to deterrence success or failure will be attenuated.

11. Sources include Brzoska and Ohlson 1987 and Stockholm International Peace Research Institute 1971, 1975a, and 1975b.

12. George and Smoke 1974 fits within this tradition while Bueno de Mesquita 1981 and Bueno de Mesquita and Lalman 1992 are the best examples of a formal modeling approach.

13. Evidence on these matters is most extensive for the United States but applies also to Israel, Britain, and other democracies (Russett 1990a, chap. 2; idem 1990b; Russett and Barzilai 1992).

14. Cross-national analyses suggest no systematic relationship between economic difficulties and international conflict in authoritarian states (resultant political dissent may be suppressed) but some relationship between overt political protest and international conflict. See Russett 1990b.

15. By operationalizing the *z*-scores on a country-specific basis, we assume that decision makers consider primarily whether political instability is high in light of their countries' own experience. We also calculated *z*-scores on the population of cases for a sensitivity check. Using the population mean in a distribution skewed toward high values would have the useful effect of focusing primarily on countries and years with quite high levels of protest and rebellion. Political leaders in countries prone to high levels of protest and rebellion may regularly see themselves as living in the domain of losses. This would lead us to expect them to be risk-acceptant —and not from individual personality characteristics.

16. Country-specific sources are too numerous to cite here, but our general sources include *Africa Contemporary Record; Africa Research Bulletin; African Recorder; Asian Recorder; Foreign Broadcast Information Service Daily Report* for China, the Middle East, North Africa, Latin America, and the Soviet Union; *Keesing's Contemporary Archives*; and the *Middle East Journal*.

17. Important work by psychologists include Janis and Mann 1977; Kahneman and Tversky 1979; and Quattrone and Tversky 1988. Influential works by political scientists, drawing on findings from cognitive psychology to analyze international security affairs, include Jervis 1976; Larson 1985; Lebow 1981; and Snyder 1984. We do not develop and test arguments from this literature focusing on the dynamics of small-group behavior or standard cognitive processes that simplify and impose order on the informational environment of decision makers.

18. Theoretically, it makes sense to consider changes in the international power balance—equally with domestic political conditions—as affecting whether a decision maker is in the domain of gains or losses. A willingness to initiate a dispute taken under conditions of uncertainty would presumably be reinforced also by the risk acceptance induced by a deteriorating international power balance; and reluctance to initiate a dispute would be reinforced also by a stable or improving international power balance. A subsequent analysis will utilize short-term trends in our indicator of the power balance for this purpose.

19. Conceptually, international, as well as domestic, conditions can produce motivated misperception (see, e.g., Lebow 1981); but we shall be able to test for only the latter here.

20. Note that we are here considering only misperceptions motivated by the pressures of domestic politics, not those that may arise from the international crisis itself.

21. Consider competing interpretations of the Iraqi attack in 1980 following the Iranian Revolution in 1979. Was Saddam Hussein seizing an opportunity or merely seeking to disarm Iranian fervor to export the revolution, a fervor magnified by the disorder in the Iranian polity and economy? (Karsh 1987).

22. Auxiliary regressions were run and multicollinearity among the measures of domestic turmoil within challenger and target was high ($> .70$) in some cases. Separate equations were run with the highly correlated domestic variables excluded, and the results were not significantly different. The signs on the coefficients remained the same, and the *t*-ratios were a bit higher; but none of the domestic variables that are statistically insignificant in Table 2 were significant in these separate equations. Autocorrelation is another potential problem. Unfortunately, we are unable to correct for autocorrelation in a probit model. We did estimate our equation with ordinary least squares, and the Durbin–Watson statistic indicated no first-order autocorrelation. This test cannot be considered conclusive, but it does suggest that autocorrelation in our data is not a significant problem.

23. The formula for the calibration index is $\mathrm{CI} = \mathrm{SUM}(\mathrm{CI}_j)/N$, where CI = calibration index, N = number of total observations, and $\mathrm{CI}_j = N_j(f_j - d_j)^2$. Where f_j = the forecast likelihood of the target event in category j, d_j = the actual frequency of target event in category j, and N_j = the number of observations in category j.

24. The marginal impact of the variables presented in Table 3 is calculated by changing the value of a single variable while holding all other variables in the model (Table 2) at their mean value. The change in the location on the cumulative standard normal distribution is then converted into the percentage change in the probability of successful deterrence.

25. Similar results are obtained when the *z*-scores for protest and rebellion levels are based on the population of cases.

26. The coefficient in Table 2 is for the variable measuring

the interaction between uncertainty and the lagged z-score for protest. The interactive variables for uncertainty and the z-scores for rebellion did not produce significantly different results.

27. Alternative measures of misperception, which utilized z-scores for levels of protest or the annual number of days of rebellion, produced coefficients with the same sign but lower *t*-ratios.

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Paul Huth is Assistant Professor of Political Science, University of Michigan, Ann Arbor, MI 48109.

Bruce M. Russett is Dean Acheson Professor of International Relation and Political Science, Yale University, New Haven, CT 06520-3532.

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IS TALK REALLY CHEAP? PROMPTING CONVERSATION BETWEEN CRITICAL THEORY AND RATIONAL CHOICE

JAMES JOHNSON *University of Rochester*

Critical theory and rational choice theory share both overlapping concerns and parallel theoretical weaknesses. Specifically, both critical theorists and rational choice theorists are preoccupied with determining what rational *can mean in the realm of social and political interaction. I show in a provisional way how game theory extends and deepens the critical theorists' basic intuition that unembellished strategic rationality cannot adequately sustain social and political interaction. And I suggest how critical theory identifies a mechanism underlying the force of the "cheap talk" that game theorists introduce in hopes of circumscribing the indeterminacy generated by their models. My goal is to stimulate productive conversation between what are typically considered discordant research traditions.*

> In fact, the first task of rational choice theory must be to circumscribe its own limits.
>
> —Elster, *The Cement of Society*

> Critical theory does not relate to established lines of research as a competitor, . . . it attempts to explain the specific limitations and relative rights of those approaches.
>
> —Habermas, *The Theory of Communicative Action*

> One seeks a midwife for his thoughts, another someone whom he can help: origin of a good conversation.
>
> —Nietzsche, *Beyond Good and Evil*

Critical theory and rational choice theory—exemplified by Jurgen Habermas' theory of communicative action and noncooperative game theory, respectively—converge in improbable but potentially productive ways at the intersection of the three epigraphs. This convergence is improbable because critical theorists and game theorists are at best indifferent to each others' work. It is potentially productive insofar as, by demonstrating *mutual* relevance and *common* concerns, it not only charts a tentative course beyond indifference but, in the process, extends the promise of enhanced theoretical understanding of social and political interaction. I hope that calling attention to the convergent tasks of critical theory and rational choice can instigate a "good conversation" between these apparently discordant research traditions. I will consequently begin by indicating in a preliminary way why this intention is not quite so farfetched as both critical theorists and game theorists might at first suspect.

Habermas explicitly locates his theory of communicative action in the lineage of critical theory represented by Theodor Adorno, Max Horkheimer, and Herbert Marcuse.[1] He nonetheless reproaches earlier versions of critical theory on three interrelated counts:

> In the first place, Critical Theory never really took the theoretical contributions of the social sciences and analytical philosophy seriously. It never engaged with them systematically, as it should have done, given its own intentions. Hence, secondly, it took refuge in an abstract critique of instrumental reason and made only a limited contribution to the empirical analysis of the over-complex reality of our society. And finally, it failed to give an unambiguous account of its own normative foundations, its own status. (quoted in Dews 1986, 49)

I will not consider whether this indictment is fair. It is more important, for present purposes, only to note that Habermas seeks to distance his theory of communicative action from the work of his predecessors on each of these counts.

Consider Habermas' response to these complaints in reverse order. First, he advances the notion of communicative reason as the centerpiece of "a social theory concerned to validate its own critical standards" (1984, xxxix). Taking strategic rationality as a counterpoint, Habermas depicts communicative reason—embodied in validity claims to truth, rightness, and sincerity that are implicitly and necessarily raised in human speech—as an unavoidable pragmatic presupposition of language use and hence of social interaction. By thus providing "a systematic grounding of the concept of reason" he hopes to establish the normative basis for his critical theory (Dews 1986, 49). Second, Habermas insists that strategic rationality cannot by itself successfully coordinate social and political interaction (White 1988, 25). In order to avoid resorting to "abstract critique," however, he anchors an encompassing theory of action in the concept of communicative reason. In this way, he aims simultaneously to concede the importance of strategic rationality and dislodge it from the center of theoretical attention. Finally, Habermas expressly distances his theoretical enterprise from the traditional self-conceptions of philosophy. He casts it instead as a "research program" intended to contribute to an empirically oriented critical *social theory* (1984, xxix, 274; 1987b, 375). In this way, he aspires to take seriously the "theoretical contributions of the social sciences," suitably disencumbered of what he considers misplaced positivist pretensions.

Given these intentions it is somewhat surprising that critical theorists do not engage game theory (surely the most systematic social scientific analysis of strategic action) in anything like a serious manner. Habermas concedes the value of game theory for understanding strategic action only in passing (1984, 86, 88n.; 1991, 242). Other critical theorists strike an even less concessive stance. If they consider the matter at all, they regard game theory as a quite foreign undertaking. Convinced of what, following Habermas, we might call the "specific limitations" of game theory, critical theorists fail to concede its "relative rights."

Thomas McCarthy, whose work is in other respects exemplary, is representative here. He announces, also in passing, that he will avoid "rehearsing the familiar debates concerning game theoretical approaches to the general theory of action" and dismissively depicts such approaches as among the "hoarier forms" of "modern social theory" (1991, 65–66). Like other critical theorists, he admonishes game theorists for reductively "conceptualizing social relations as strategic relations and social interaction as strategic interaction" and seems not to consider what they might learn from such an exercise (ibid.).

Despite such resistance, critical theorists might indeed learn from noncooperative game theory.[2] The latter constitutes a thoroughgoing attempt to probe the nature and limits of strategic rationality by reconstructing the performance of strategically competent actors in settings where binding communication is precluded.[3] The unintended upshot of this effort is to demonstrate systematically what critical theorists suspect, namely, that in many settings strategic rationality alone does not suffice to sustain social relations. In fact, formal game-theoretic results show that in dynamic settings, strategic interaction generates widespread indeterminacy in the form of multiple equilibria and attendant coordination problems. This disquieting conclusion, especially in light of how some game theorists seek to remedy it by incorporating communication into their models, in turn provides warrant for suspecting that something like communicative reason as Habermas envisions it exists and functions to coordinate social and political interaction.

This prospect might well entice critical theorists into a conversation with game theorists. But what of game theorists? What might they learn from the theory of communicative action? The answer to these questions comes into sharper focus if we recall the problem of whether and how rational choice theory might circumscribe its own limits. The "scope of game theory itself is challenged only when a critic calls for consideration of a factor that is intrinsically impossible to represent in a game theoretic model" (Myerson 1992, 66). Leaving aside, for now, vagaries surrounding the phrase *intrinsically impossible*, the indeterminacy manifest in multiple equilibria seems to mark one important limit to game theory (Kreps 1990a, 95–102; Myerson 1992, 67–68). As just mentioned, game theorists themselves try to build communication into their models as a way of circumscribing indeterminacy (Crawford 1990). But they do not offer a compelling account of the force of communication in coordinating interaction. Critical theorists not only propose a mechanism underlying the force of communication but do so in ways that in important respects, are consonant with game theorists' enterprise.

My argument will be provisional, following a conversational pattern that focuses first on weaknesses in the theory of communicative action, then turns to limits of game theory, and finally (with these limits and weaknesses in mind) explores the terrain on which a conversation between the theory of communicative action and game theory might be joined. My intention, however, is not to simulate a conversation between critical theorists and game theorists. It is to provide reasons why they might pursue one themselves.[4]

VALIDITY CLAIMS IN THE THEORY OF COMMUNICATIVE ACTION

Habermas seeks to establish normative foundations for critical social theory in a "comprehensive concept of communicative rationality" (1984, 14). He insists that rationality is not a disembodied notion, that it "has less to do with the possession of knowledge than with how speaking and acting subjects *acquire and use knowledge*" (ibid., 8; idem 1987a, 314). In particular, he claims that the primary criterion for ascertaining the rationality or otherwise of an action is whether it can be defended in the face of criticism (1984, 16).

Habermas anchors his entire theoretical enterprise in a categorical distinction between two sorts of purposive social action (1984, 84–101, 286–87).[5] Strategic action, on his account, is oriented toward success. It coordinates interaction via influence. Communicative action, by contrast, operates in the medium of language and is oriented toward reaching understanding. It coordinates interaction via consent or rational agreement. Parties to communicative action aim cooperatively to negotiate shared understandings of the nature of their interactions.

In communicative action, competent speakers raise and respond to "exactly three criticizable validity claims"—truth, rightness, and sincerity—depending on whether they are taking up a relation to the objective, social, or subjective world (Habermas 1984, 99, 307–8; idem 1991, 314). In everyday communicative practice, they do so naively and implicitly; in argument or discourse, they do so reflectively and explicitly. Communicative action derives its force from the potential for rational agreement embodied in validity claims.[6] More specifically, it derives force from the guarantee, necessarily extended by competent speakers, to redeem, in the event they are challenged, the validity claims raised by their utterances (Habermas 1984, 302; idem 1985, 170; idem 1990, 58–59). This guarantee represents the "telos of mutual understanding" that Habermas claims is inherent in human communication (Dews 1986, 99).

Habermas insists that the process of raising and responding to validity claims is not a contingent aspect of language use (1991, 238). It constitutes the unavoidable *pragmatic* presupposition of communicative interaction.[7] In this sense, competent speakers have no alternative. Any social actor who attempts consistently to deny that his or her utterances raise validity claims faces a "performative contradiction." The very denial presumes criteria of valid argumentation in terms of which it can be recognized as such.[8]

This is the barest sketch of Habermas' complex argument. From it, however, one can see that validity claims are "central" to the idea of communicative action (Habermas 1984, 10). This sketch also raises a fairly obvious question. What sort of argument does Habermas advance for the existence of validity claims? Habermas' entire project rides on his ability to answer this question persuasively. His theory is plausible only to the extent that he can demonstrate how rational agreement or consent, operating in the medium of language, coordinates social interaction (ibid., 278, 298; idem 1985, 169–70). And agreement or consent, in turn, emerges because speakers mutually recognize the binding force of the validity claims raised in their speech acts (1985, 153; 1987b, 120).

Habermas uses speech act theory to explicate the concept of communicative action and to set it off from strategic action. With certain important qualifications, he differentiates communicative and strategic action in terms of the illocutionary aims pursued in the former and the perlocutionary effects sought through the latter (1984, 295). His argument on this score has been forcefully criticized, and I will not reiterate those complaints here.[9] The difficulty, for present purposes, is that Habermas' analysis of speech acts is not itself an argument for validity claims.[10] Instead, it presumes that validity claims exist and function to coordinate social interaction: "Validity claims . . . give the illocutionary act a rationally motivating force" (Habermas 1979, 65; idem 1984, 304).

Habermas, then, presumes—but does not show—that there is a telos of understanding built into the validity basis of human speech. Not surprisingly, critics ask to be convinced of this (Wood 1985). It is, however, important to note that Habermas actively engages his critics to a remarkable degree and, in the process, attempts to clarify or remedy what they take to be troublesome aspects of his work (e.g., Habermas 1991). So while there is perhaps reason to be skeptical of his theory as currently formulated, there also is good reason to suspect that he will continue to address its current weaknesses. I hope (1) by taking game theory seriously, to sharpen and extend Habermas' insight that strategic rationality cannot by itself coordinate social and political interaction, and (2) to dispell skepticism about the more constructive dimensions of Habermas' critical theory by suggesting that communication, rather than some more standardly recognized mechanism such as social norms, functions to coordinate social and political interaction.

THE IDEA OF STRATEGIC COMPETENCE

Habermas populates his theory of communicative action with actors who are competent language users. He presumes, that is, that actors are "communicatively competent" in the sense that they have a generalized capacity not simply to produce grammatical sentences but to raise and respond to the validity claims implicit in everyday language use (1979, 26–29). This, however, is only one among several sorts of "interactive competence" that he attributes to social actors.[11]

Game theorists, by analogy, attribute to actors a "unique human capacity for strategic behavior" (Elster 1979, 2, 9–18). They populate their models with actors who are *strategically* competent, who are capable of engaging in strategic rather than merely instrumental or parametric action.[12] Game theorists, that is, presume that social actors can (1) understand that their environment partly consists of other intentional agents and (2) recognize those others as equally rational. These generalized capacities enable social actors to proceed strategically by attempting to accommodate the anticipated actions of relevant others when formulating their own plans.

Habermas, too, presumes that social agents are strategically competent in something like the sense just sketched. He implies this in his typology of action and in his remarks on the scope of reconstructive theory (see my earlier work, Johnson 1991a, 1991b). But he perhaps most clearly relies on the notion of strategic competence in his account of the development of "moral consciousness" defined as "the ability to make use of interactive competence for *consciously* processing morally relevant conflicts of action" (Habermas 1979, 88). He has in mind, specifically, the ability of social actors discursively to examine the validity—and thereby potentially establish the legitimacy—of established norms. Yet Habermas shows how the very capacity to engage in normatively governed action that such practical discourse presupposes can emerge only in intimate relation to the development of strategic competence.

Habermas uses the theory of communicative action as a framework to reconstruct the interrelated processes of social and moral development that generate mature moral consciousness. Elaborating on work by developmental psychologists J. H. Flavell, Lawrence Kohlberg, and Robert Selman, he views this as a complex learning process in which the child's chronic inability successfully to navigate particular sorts of interaction precipitates the transition from one developmental stage to the next. Habermas portrays development as a dynamic process of "creative reorganization" in which capacities constitutive of prior stages are taken up and transformed in the transition to subsequent stages (1990, 125).

The point at which, according to Habermas, strategic competence plays a decisive developmental role is with the transition from the preconventional to the conventional stage of interaction. In part, this transition consists in "the restructuring of preconventional competitive behavior into strategic action"; at this juncture the child becomes able to appreciate how his own actions are situated in a web of social interdependencies: "At the conventional level the characteristic innovation was the actor's ability to view himself in reciprocal relation to others as a participant in a process of action and *at the same time* to step outside and observe himself as a constituent part of interaction" (Habermas 1990, 159). Moreover, the child begins to recognize others as rational agents: "Alter stops being perceived as someone whose actions are determined by shifting needs and interests and is now perceived as a subject who intuitively follows rules of rational choice" (p. 150). With the transition from the preconventional to conventional stage, therefore, the child becomes strategically competent.

Habermas portrays the development of strategic competence as a necessary precondition of the child's emerging capacity to engage in normatively regulated interaction. He summarizes this point as follows:

> the conventional stage of interaction is characterized by the rise of two contrasting types of interaction: strategic action and norm-governed interaction. Owing to the integration of the observer perspective into the sphere of interaction, the child learns to perceive interactions, and his own participation in them, as occurrences in an objective world. This makes possible the development of a purely success oriented type of action as an extrapolation of conflict behavior guided by self-interest. At the same time that strategic action is being acquired through practice, its opposite, non-strategic action, comes into view. Once the *perception* of social interaction is differentiated in this way, the growing child cannot avoid the necessity of also reorganizing types of nonstrategic action that had been left behind in his development, so to speak, to bring them into line with the conventional level. What this means is that a social world of norm-guided interactions open to thematization comes to be set off against the background of the lifeworld. (Habermas 1990, 140)[13]

As I understand Habermas' account of this dynamic, the capacity to engage in normatively regulated action both presupposes the emergence of strategic competence and compels further reorganization of prestrategic competitive behavior (pp. 141–52). I will not examine the details of his discussion. My point here is simply to indicate that "the transformation of interest-governed conflict behavior into strategic action" is among "the structural preconditions of communicative action" (p. 158).

TWO DIFFICULTIES

Strategic competence is, on Habermas' account, crucial to the development of full moral consciousness. This provides critical theorists with a forceful reason to take seriously game-theoretic accounts of how strategic competence manifests itself in performance. We can fortify this reason by considering two difficulties that the idea of strategic competence reveals in the theory of communicative action.

First, Habermas' account of sociomoral development recapitulates a basic deficiency in his theory. Specifically, his categorical distinction between action oriented to success and action oriented toward reaching understanding is insufficiently nuanced. In his typology of rational action, for example, this misleads Habermas into depicting strategic action as essentially egoistic and atomistic (Johnson 1991a). In the present context, it similarly prevents him from fully appreciating the degree to which "strategic action" actually "comes to be *differentiated* from competitive behavior" in the developmental process (Habermas 1990, 150; my emphasis). This shortcoming manifests itself as a crucial ambiguity at the center of Habermas' reconstruction of sociomoral development.

Habermas insists that in developmental models such as his own, the transition from one stage to another involves thoroughgoing reorganization of the principles governing the earlier stage; and he sharply criticizes Kohlberg for failing to demonstrate how the latter's theory of moral development articulates such a stage structure (Habermas 1990, 127–28). Yet, in the long passage just cited, Habermas depicts the emergence of strategic competence as simply an "*extrapolation* of conflict behavior guided by self-interest." He then proceeds to elucidate the transition from preconventional competition to conventional strategic action by reference to an experimental study reported by J. H. Flavell that captures a pure-conflict, constant-sum game (ibid., 148–50).

At a critical point in his treatment of moral development, then, Habermas mistakenly conflates strategic interaction with constant-sum, pure-conflict situations. Here, as elsewhere in his theory, Habermas obscures the by now commonplace observation that mixed motivations are central ingredients in most strategic interactions (Schelling 1960, 88–89). This reinforces the overly stark opposition he draws between strategic and communicative action. And because it leads him to overlook the particular ways that strategic actors presuppose a broader structure of communication in order to coordinate social and political interaction successfully, it prevents Habermas from consistently appreciating that strategic action is a form of *social* action (Johnson 1991a).

In a pure-conflict, constant-sum setting, each player is driven by self-interest to minimize the maximum payoff to the opponent, thereby ensuring himself the greatest possible return. In more routine variable-sum interactions, by contrast, players have mixed motives. The return to each depends, to some greater or lesser extent, on whether, and how successfully each is able to coordinate with relevant others.[14] This need to coordinate renders variable-sum interactions truly strategic. It also establishes a crucial role for communication in strategic interaction. The contrast with zero-sum settings is striking.

> In a zero-sum game the analyst is really dealing with only a single center of consciousness, a single source of decision. True, there are two players, each with his own consciousness; but minimax strategy converts the situation into one involving two essentially unilateral decisions. . . . No social perception is involved. But in a mixed-motive game, two or more centers of consciousness are dependent on each other in an essential way. *Something has to be communicated.* (Schelling 1960, 163; emphasis mine)

Game theorists have struggled in recent years to account for how communication operates as a mechanism for coordinating social and political interaction. I claim in later sections that this provides the ground upon which critical theorists and game theorists might join in productive conversation.

Second, Habermas himself mentions an especially germane difficulty that his presumption of strategic competence underscores. He notes that—like "any approach that distinguishes competence from performance"—his reconstruction of sociomoral development raises a vexing methodological problem. In particular:

> Such theoretical approaches face specific measurement problems because competence can be captured only in its concrete manifestations, that is, only in performance. Only insofar as these measurement problems have been solved can we isolate factors determining performance from theoretically postulated competences. It may be helpful to distinguish factors determining performance that, as *stimulators* or *accelerators*, must *supplement* or can accompany an acquired competence from the *braking* and *inhibiting* factors that act as filters. (Habermas 1990, 187)

The notion of strategic competence provides an instructive basis for exploring the implications of this statement. Consider two sets of experimental findings.

On the one hand, developmental psychologists have determined that as early as six to nine years of age children become able both to entertain second-order states (beliefs about beliefs, intentions about beliefs, etc.) themselves and to attribute such states to others. As the psychologists recognize, this capacity for recursive thought is an indispensable prerequisite of strategic rationality (Perner 1988; Perner and Wimmer 1985).

On the other hand, experimental evidence also suggests that even in relatively elementary negotiations, mature actors frequently do not avail themselves of their capacity for strategic thinking. Experimental subjects, that is, proceed parametrically, rather than strategically. They do not recognize others as equally rational agents whose intentions must be anticipated and accommodated as the subjects themselves formulate their own bargaining strategies (Bazerman and Carroll 1985, 252, 258–59). This sort of parametric interaction frequently generates outcomes that are undesirable from both individual and collective perspectives.

Taken together, this pair of experimental results highlights the complexities involved in identifying the "braking and inhibiting factors" that might interfere with how strategic competence manifests itself in performance. Critical theorists would likely attribute the sorts of distorted interaction reported in the second set of experimental results to the influence of what Habermas calls "non-generalizable interests" (1984, 35). The psychologists who conducted the experiments advance a rival interpretation. They conclude, instead, that experimental subjects resort to parametric thinking in an effort to render bargaining situations cognitively tractable (Bazerman and Carroll 1985, 260).

This sort of conflict of interpretation is theoretically disconcerting. However, by appropriating game-theoretic insights, critical theorists could substantially mitigate such difficulties. To paraphrase Habermas, formal game-theoretic models capture the performance of strategically competent actors. In this sense, game theory is best understood as a reconstructive theory of strategic rationality (Johnson 1991b). Reconstructive theories, according to Habermas, provide formal analyses of the basic interactive competences of social actors (1979, 8–14; 1990, 21–42). They "acquire a *critical* function" to the extent that they "explain deviant cases" (idem 1990, 31–32). Critical theorists, for example, frequently assert that nongeneralizable interests distort or inhibit social interaction. Well-known game-theoretic results function critically by sustaining such assertions. Indeed, game theorists identify entire classes of strategic interaction that generate inefficient equilibria because relevant parties act solely out of narrow self-interest.[15]

Critical theory, however, cannot content itself with reconstructing "deviant cases." The very notion of deviance implies a background of standard cases free, at least in principle, of distorting factors. In that sense, a critical reading of game theory, too, must discharge a "theoretical" function by facilitating formal analysis of standard strategic interaction (ibid., 32). At this level, however, critical theorists confront a rather peculiar situation, because formal game-theoretic models provide insight into the systematic limits of strategic rationality.

Game-theoretic results not only demonstrate that strategic competence is somehow distorted, as in the deviant cases mentioned. They also show how pervasive indeterminacy besets the strategically competent actors who populate game-theoretic models even where, as in coordination problems, the "braking and inhibiting" effects of nongeneralizable interests are minimized or bracketed entirely. This indeterminacy ensues because players face "strategic uncertainty" in the sense that they each know all of the options available to relevant others but do not know which option those others will actually pursue.[16] It is this sort of uncertainty, for example, that the investigators invoke to explain why the subjects in the bargaining experiments reported earlier resort to parametric, rather than strategic, thinking. But the theoretical importance of strategic uncertainty extends well beyond those particular experimental results. It suggests that strategic competence is a precarious achievement, susceptible not only to the distorting pressures of selfish motivation but also to endogenous limits, namely, the persistent indeterminacy that derives from strategic uncertainty.

Game theorists have devoted considerable energy to the task of tracing the implications of this insight in their models. They do not simply demonstrate *that* strategic rationality alone cannot adequately coordinate social and political interaction, but they demonstrate more systematically and precisely *why* this is so. In so doing, game-theoretic results extend the basic negative insight from which Habermas proceeds in a way that makes it deeper and more incisive.

INTERPRETING GAME THEORY

I shall now advance a compressed and deliberately partial history of game theory.[17] I focus almost exclusively on the limits of game theory and especially on the indeterminacy that many game-theoretic models generate. A more balanced account would obviously require greater appreciation of the range of situations in which game theorists demonstrate that strategic rationality can coordinate social and political interaction (Kreps 1990a, 37–91). With that caveat in mind, game theorists will not, I hope, find my admittedly incomplete record of their enterprise entirely alien.

Game theorists, like rational choice theorists more generally, proceed from a presumption of rationality (Elster 1979, 116–17). They presume, that is, that actors are rational in the minimal sense that they are purposive and, in most situations of consequence, consistent.[18] Thus, when game theorists encounter apparently irrational behavior, this methodological presumption directs them to scrutinize the broader situation or context for an indication of how the perplexing action might be construed as rational.

Situations of strategic interdependence like those captured in game-theoretic models unexpectedly complicate this methodological stricture. In strategic settings, each agent's action is contingent upon the action of relevant others. Because, for each agent, the context of interaction is constituted partly by other intentional agents, it cannot be treated as a parameter. As a result, the apparently straightforward idea of rationality no longer provides solid theoretical moorings.

> Game theory is the formal study of the rational, consistent expectations that participants can have about each other's choices. It is . . . not the empirical study of how people make decisions but a deductive theory about the conditions that their decisions would have to meet in order to be considered "rational," "consistent," or "noncontradictory." Of course defining "rational," "consistent," or "noncontradictory," for interdependent decisions is itself part of the business of game theory. (Schelling 1967, 215)

Faced with this task, then, "The theory of games . . . does not assume rational behavior; rather it attempts to determine what 'rational' can mean when an individual is confronted with the problem of optimal behavior in games and equivalent situations" (Morgenstern 1968, 62). In strategic situations, as Schelling and Morgenstern testify, the concept of rationality itself calls out for scrutiny. The objective becomes "to determine what 'rational' can mean."

Given this conception of their undertaking, game theorists rapidly encounter profound conceptual difficulties. Indeed, one critic concludes that

> game theory addresses itself to the . . . problem that arises whenever an . . . actor takes into account the possible reactions to his own decisions of the other actors. To my mind, the main product of the very elegant apparatus of game theory has been to demonstrate quite clearly that it is virtually impossible to define an unambiguous criterion of rationality for this class of situations (or what amounts to the same thing, a definitive definition of the "solution" of a game). (Simon 1979, vol. 2, 486–87)[19]

This observation goes to the core of game theory. In order to grasp its force, we need to be clear about some elementary technical concepts.

In game theory, a "solution" consists of a definite prediction regarding what strategically rational players will do in the situation being modeled (Kreps 1990a, 29–30). Initially, it was supposed that this consisted largely of identifying an equilibrium point that would terminate the potentially infinite regress of reciprocal expectations unleashed as players sought to anticipate what other players would do and adjust their strategies accordingly (Elster 1979, 19). In game theory, an "equilibrium" identifies a strategy combination specifying each player's best response to the anticipated actions of relevant others. Once reached, an equilibrium is self-enforcing in the sense that no player has any incentive to depart unilaterally from his or her equilibrium strategy.[20]

An equilibrium point, however, is only a necessary—not a sufficient—condition for the existence of a solution. This is because a game might admit either of no equilibrium or of several (Elster 1979, 117–23).[21] For present purposes, consider the problem of multiple equilibria:

> The difficulty lies in the fact that almost any interesting non-cooperative game—including almost any interesting non-cooperative bargaining game—will have a great many, and often infinitely many, very different equilibrium points. . . . This means that, if all we can say is that the outcome of the game will be an equilibrium point (or even that it will be a perfect equilibrium point), then we are saying little more than that almost anything can happen in the game. (Harsanyi 1977a, 102)

In short, the world captured by noncooperative game theory is plagued by an indeterminacy that, if not total, is pervasive. And game theory itself provides scant assistance in coming to terms with the indeterminacy it generates (Kreps 1990a, 97).

Perhaps the best-known example of this indeterminacy is the so-called "folk theorem" for repeated games.[22] This theorem states that provided players accord sufficient weight to future interactions, any combination of strategies that ensures each player a payoff greater than could be obtained by proceeding unilaterally can be sustained as an equilibrium. In an iterated prisoner's dilemma, for instance, it is well known that dynamic considerations can induce players to cooperate. The folk theorem suggests that in such situations repetition also fosters indeterminacy in the form of a "bewildering wealth" of equilibria on which players might potentially converge (Aumann 1981, 16).

Confronted with this sort of disquieting result, game theorists have a limited number of options:

> A model with no equilibrium or with multiple equilibria is underspecified. The modeler has failed to provide a full and precise prediction of what will happen. One option is to admit that his theory is incomplete: an admission of incompleteness like the Folk Theorem . . . is a valuable negative result. . . . Another option is to renew the attack by changing the game's description or the solution concept. (Rasmusen 1989, 27)

Technical game theorists have primarily pursued the second option by devising new solution concepts.[23] They have advanced several "equilibrium refinements" intended to circumscribe the range of indeterminacy in their models.[24]

The details of this technical work are unimportant here. What is significant is the rapid proliferation of solution concepts it has generated.

> The situation is particularly confusing in respect of the noncooperative analysis of games with some dynamic structure in which the choice of one move or another during the play of the game may convey valuable information to the other players. Without pausing for breath it is easy to name at least ten rival equilibrium notions for which a serious case can be made that here is the "right" solution concept for such games. (Binmore 1990, 151)

This, as I understand it, is the current state of the art in noncooperative game theory (Kreps 1990a, 108–28). The indeterminacy revealed in the technical literature has elicited a variety of solution concepts that reflect divergent, not entirely convincing notions of "what 'rational' can mean" in strategic settings.

My intent in recounting this attenuated history of game theory is not to suggest that it is impossible *in principle* that game theorists might bring some order to the "confusing" theoretical situation in which they find themselves. Rather, I want to suggest that it dramatically underscores the importance of the *first* option Rasmusen mentions. It is plausible to treat the pervasive indeterminacy generated in game-theoretic models as what he calls "a valuable negative result."[25] More specifically, it is plausible to interpret this indeterminacy as identifying, however provisionally, the limits of strategic rationality as a mechanism for coordinating social and political interaction.

On this interpretation, game theorists have implicitly begun to chart the limits not only of strategic rationality but of their own theory, as well. This suggestion is not unprecedented. Some years ago, Anatol Rapoport observed, "The great philosophical value of game theory is in its power to reveal its own incompleteness. Game-theoretical analysis, if pursued to its completion, perforce leads us to consider other than strategic modes of thought" (1966, 214). The point here is not that actual social and political practices generate indeterminate outcomes but that in the austere world captured by formal game-theoretic models, unembellished strategic rationality does so (Elster 1989, 85). And this, as Rapoport suggests, prompts the suspicion that nonstrategic factors account for the discrepancy between theoretical indeterminacy and more settled social and political practice.

IS TALK REALLY CHEAP?

This is the juncture where a conversation between critical theory and game theory can be joined in earnest. Game theorists might counter the chronicle I have presented by claiming that insofar as their theory at least begins to specify its own limits, they need not rely on critical theory to perform that task. They might, for instance, subscribe to the following estimate of their theoretical predicament: "All this may sound very slippery and unsatisfactory. There are no firm predictions, no falsifiability. If our theory appears not to work, we don't lose any sleep. 'Rationality is just one of the relevant factors,' we say blandly; 'here something else was at work'" (Aumann 1985, 37). Rather than endorse this impassive posture, I want to suggest in a preliminary way that critical theorists might advance a distinctive interpretation of game-theoretic results by identifying what, beyond strategic rationality, is at work in coordinating social and political interaction. The "something else" that they identify, of course, is precisely the binding force of speech acts.

In his theory of communicative action, Habermas seeks to reconstruct "institutionally unbound" speech acts, that is, utterances deriving force as mechanisms for coordinating social interaction solely from universal validity claims, rather than from the contingent normative or institutional context in which they are advanced (1979, 38–40, 60–61; 1984, 295, 440, n. 40). So, too, noncooperative game theory reconstructs "institutionally unbound" strategic action. It aspires to identify equilibrium outcomes sustained solely by the choices of strategically rational actors. For that purpose, it standardly treats institutions, culture, and so on as "incidental" or "inessential" detail and relegates them to the "'boundary conditions' that tie the modeled structure to its unmodeled environment" (Schelling 1960, 76, 106; Shubik 1982, 11–13). Game theory demonstrates how strategic interaction in so stark an environment generates rampant indeterminacy. It thus makes conspicuous the need to identify additional, nonstrategic mechanisms that coordinate social and political interaction.

Critical theorists identify just such a mechanism—the binding force of validity claims raised in communicative action.[26] Game theorists have indeed introduced communication between players in an effort to circumscribe the indeterminacy generated by their models. The problem is that noncooperative game theory by definition captures a hypothetical world where even such communication as is allowed is nonbinding. This obviously imposes a severe constraint on the effectiveness of communication in game-theoretic models. It also affords critical theorists an opening from which to advance a distinctive interpretation of the limits of game theory.

The players who populate game-theoretic models have access to two analytically distinct types of communication (Farrell 1990, 3; Kreps 1990b, 388–89).[27] The first type consists of sometimes tacit, always costly signals that are rather easily modeled as moves in a game-theoretic framework. It is a form of what Habermas calls "linguistically mediated strategic action," in which players concerned with their own success seek for that purpose to influence the choices of relevant others (1984, 295). Since any such message a player communicates is directly colored by his preferences over outcomes, signaling raises vexing questions of credibility that inhibit its capacity to coordinate interaction. Indeed, as noted earlier, the rival and not entirely compelling equilibrium refinements that game theorists advance are designed to circumscribe, to some extent, the chance that players will either convey or believe incredible messages (Farrell 1990, 4). This sort of communication thus serves, in the first instance, as much to accentuate, as to resolve, the theoretical difficulties that currently beset game theory.

The other type of communication available to players in game-theoretic models differs from the costly signals just discussed in two ways. First, it consists of talk that game theorists deem *cheap* on the grounds that since any information it conveys is costless and unverifiable, it has no *direct* bearing on players' payoffs (Crawford 1990; Schelling 1960, 117). Second, it fits rather uneasily into game-theoretic models. Because cheap talk takes place during one or more rounds of *preplay* communication, it resists exhaustive formalization (Kreps 1990b, 388–89). Despite these apparent difficulties, however, experimental evidence and, more importantly, some recent formal results indicate that even in austere game-theoretic environments, cheap talk has unexpected effects (Crawford 1990).[28] In game theoretic models of bargaining, for example, strategic uncertainty generates multiple equilibria and attendant coordination problems.[29] In such settings cheap talk functions, within limits, to coordinate players' expectations (ibid.).

Game theorists simply lack the conceptual resources to account for the binding force of cheap talk.[30] While they recognize that it seems to coordinate expectations effectively, they are at a loss to explain how it does so. In their idiom, talk is cheap precisely because it derives force from neither of the sources to which they typically attribute social and political coordination: self-enforcing equilibrium outcomes generated by individual choice or else some sort of contingent, exogenous enforcement.

Critical theory circumvents this theoretical predicament. Habermas, in effect, asks, "Is talk *really* cheap?" He answers *no*, not because communication is linked to payoffs or to exogenous sources of enforcement but because of the binding force of utterances per se. At this juncture, Habermas clearly pushes inquiry beyond the terrain of game theory. It is important, however, to note two things. First, game theorists themselves have escorted Habermas this far. They not only chart the limits of strategic rationality but incisively identify the nature of those limits in ways that critical theorists do not. Second, while critical theory and game theory seem to diverge sharply here, the resolution Habermas implicitly proposes to the game theorists' predicament illuminates crucial aspects of their research agenda.

This last point becomes clear if we consider two analytically distinct factors that conspire to circumscribe the effectiveness of cheap talk in game-theoretic models: comprehensibility and credibility. Comprehensibility is, for game theorists, the analytically "more fundamental," though the less familiar, of the two factors (Farrell 1990, 2). If players rely on cheap talk to coordinate their expectations, each must, in some minimal sense, be able to formulate messages that relevant others will understand. Consequently, when game theorists incorporate cheap talk into their analyses, they tacitly attribute "linguistic competence" to each actor in their model. They presume, that is, that the actors populating their models have mastered the grammatical rules of a shared natural language and that this mastery enables them to produce "comprehensible" sentences (Habermas 1979, 20, 26–27).

The problem for game theorists is that this assumption introduces an added source of indeterminacy into their models. Linguistic competence is a source of creativity. It consists of the capacity not simply to formulate comprehensible utterances but to do so in innovative ways and under novel circumstances. This complicates game-theoretic models insofar as it enables players to formulate *neologisms*, or unanticipated messages that are not used in equilibrium. In the context of a common natural language, neologisms can, under certain conditions, acquire a focal quality that undermines what might otherwise prove to be the equilibrium expectations of relevant players (Farrell 1990, 6–9).

Given the availability of neologisms, credibility, the second factor that constrains the effectiveness of cheap talk, confounds matters further still. Even in mildly mixed-motive interactions like those modeled as coordination problems, the divergent interests of relevant players offer an incentive to dissemble or misrepresent (Crawford 1990, 216–17; Farrell 1990, 1).[31] Specifically, each relevant player has an incentive, however slight, to invoke an available focal neologism in hopes that doing so will destabilize extant equilibrium expectations and induce another more to their liking.

Game theorists respond to these destabilizing possibilities by specifying conditions under which equilibria, because they are "neologism-proof," remain viable (Farrell 1990, 2–3, 8–10).[32] This response raises familiar difficulties. A neologism-proof equilibrium does not exist in every cheap talk game; and where one does exist, it need not be unique. More importantly, however, the conceptual strategy underlying this refinement is curious. Having implicitly endowed the players who populate cheap talk models with linguistic competence, this response ironically seeks to rein them in by, in a sense, stifling the creativity that their competence makes possible. Players remain capable of formulating neologisms, but the range of viable equilibria in a game consists only of those immune to their destabilizing effects.

Habermas approaches this problem in an instructively dissimilar way. The game-theoretic strategy just discussed seeks to mitigate credibility problems by circumscribing the potential for innovation inherent in linguistic competence. It imposes constraints on the effectiveness of potentially destabilizing neologisms that players might formulate. Habermas, instead, seeks to constrain the nongeneralizable interests that provide players with incentives to dissemble or misrepresent. He, too, presumes that social actors are linguistically competent. But as noted earlier, he also presumes that they are *communicatively* competent. He insists that they are capable not only of producing comprehensible utterances but of embedding their utterances, however unanticipated they may be, in a system of criticizable validity claims to truth, rightness, and sincerity (Habermas 1979, 26–27). This further competence sustains processes of contesting and redeeming validity claims that impose pragmatic constraints on the ability of players to make utterances that solely express their nongeneralizable interests.[33] It thus restricts the sorts of reasons players might provide in defense of any neologism they formulate. Here Habermas identifies what game theorists lack—a mechanism that might compellingly account for the binding force of language in strategic interaction.

On this reading, then, the theory of communicative action can account for the effects of what game theorists call cheap talk. Yet the account it offers, while suggestive, is incomplete; for if, as this reading suggests, the system of contestable validity claims that lends speech acts binding force provides a plausible mechanism for grasping the force of cheap talk, it also raises a series of pressing issues. I will mention only two. Most obviously, critical theorists need to provide a more precise idea of the conditions under which language can be expected to coordinate social interaction successfully. For example, communicative action remains susceptible to pressures of nongeneralizable interests. This is perhaps clearest in pure-conflict, zero-sum interactions where communication, including cheap talk, has no force.[34] But even in more auspicious circumstances, where actors need to communicate in order to coordinate their expectations, there is no guarantee, even at the reflexive level of discourse, that parties to communicative action will reach agreement.[35] More fundamentally, suggesting *that* the validity basis of language works to coordinate social interaction is not the same as establishing *how* it does so. And, as noted earlier, critical theorists lack a persuasive account of the latter.

Habermas' theory of communicative action presently sheds little light on theoretical issues of this sort. This may seem to vitiate his accomplishment. But this conclusion would be, at best, misleading. Consider the relative purchase that game theory and the theory of communicative action afford us in conceptualizing the binding force of cheap talk. Each theory identifies factors that disrupt that force. But while game theory provides no account of why cheap talk can ever succeed, critical theory identifies a mechanism to explain how, within constraints, it might coordinate social and political interaction. If critical theorists have considerable work to do and have at hand only some of the conceptual resources necessary to the task, game theorists are at a loss over where and how to begin.

CONCLUSION

My arguments are intentionally preliminary. I present them in hopes of initiating a good, if unlikely, conversation between critical theorists and game theorists. Skeptics may complain that I do not report any "results." But I anticipate that a conversation of the sort I envision would be productive, as well as edifying. Where, then, do my arguments leave the respective parties to this hypothetical conversation?

A too simple reading might take my arguments as vindicating critical theory. This reading might proceed as follows. Both critical theory and game theory are preoccupied with determining "what 'rational' can mean" in the realm of social interaction. Game theorists offer a robust analysis of strategic interaction. In the process, they plot the dual limits of strategic rationality and of their own formal reconstructions of it. Habermas counsels against the sort of impassive position they seem ultimately to adopt in the face of these limits. But because, like them, he is concerned with "institutionally unbound" interaction, he resists resorting to exogenous sources, such as social norms, culture, or institutions as coordinating mechanisms. Instead, he reconstructs a comprehensive theory of rationality anchored in the concept of communicative action. The resulting theory of communicative action enables Habermas to specify the limits of game theory. It also provides the vantage point from which he aspires simultaneously to validate the normative presuppositions of his critical theory and assess the legitimacy or otherwise of prevailing social and political arrangements.

This reading is, indeed, overly simple. It suggests far greater closure than my arguments actually warrant. First, game theorists systematically explore the nature and limits of strategic rationality in ways that do not just bolster, but extend and deepen, critical theorists' conviction that it cannot by itself sustain social and political relations. The former's results thus highlight the poverty of the latter's understanding of strategic action. Moreover, the sketch of game theory that I present is partial. It provides no reason to presume that game theorists are incapable of further clarifying the "confusing" state of their discipline and thereby broadening the range over which we might come to expect strategic rationality to coordinate social and political interaction effectively.

Second, following Habermas, critical theorists might pursue a persuasive resolution to the predicament that currently besets game theory. But this resolution surely is provisional. It might be challenged by game theorists themselves and perhaps by others who advance rival accounts of the mechanisms underlying discussion-induced cooperation. Moreover, nothing I have said here definitively establishes the strongest of Habermas' claims, namely, that there is a telos of understanding intrinsic to human language; that this telos is embodied in a system of criticizable validity claims; that these represent the unavoidable pragmatic presuppositions of human interaction; and that, therefore, in social interaction communicative reason is necessarily prior to strategic rationality. In order adequately to defend his project as he currently formulates it, Habermas must justify these claims in a more compelling way than he has to date (Johnson 1991a). My arguments simply suggest that attempting such a task is not nearly so preposterous as critics sometimes make out.

Finally, game theorists will surely be apprehensive about the conversation I propose. If overcoming impassivity demands endorsing Habermas' ambitious enterprise in its entirety, they might reasonably remain skeptical. Yet critical theory itself is not necessarily committed to his entire agenda. Even sympathetic observers suspect that "Habermas' conceptions of reason and rationalization, theory and discourse . . . are stronger than his arguments warrant or his project requires." This suspicion is surely sound. And proposals to remedy it by pursuing "an alternative 'weaker' program for critical theory" are attractive (McCarthy 1991, 3).[36] A constructive conversation between critical theorists and game theorists might illuminate this more modest agenda. Whether and how it might then be realized remains to be seen.

Notes

Thanks to Jack Knight, Joe Heath, Jenny Mansbridge, and J. Donald Moon for comments on earlier drafts. Jack, in particular, once again helped me see, if not entirely overcome, the shortfall from thinking one has made an argument to actually making it.

1. For a critical overview of this earlier work that connects it with Habermas' concerns, see Held 1980 and Honneth 1987. For the broader contours of Habermas' own project, see McCarthy 1978 and White 1988.

2. Game theorists distinguish between cooperative and noncooperative games. The crucial difference is that the former allow for *binding* communication between players, while the latter do not (Binmore 1990, 32). Game theorists consider noncooperative games to be analytically fundamental (Harsanyi 1977a, 92; Rasmusen 1989, 29). Consequently, I focus my remarks on the theory of noncooperative games.

3. Philosophers frequently evaluate economic models, including game-theoretic models, solely on the criterion of empirical performance. However, such "concern with problems of empirical appraisal is exaggerated" and fails to "recognize that the activities of formulating economic models and investigating their implications are a sort of conceptual exploration" (Hausman 1989, 115). On the reading I present, game-theoretic models are precisely formal explorations of our standard concept of strategic rationality. This is an idiosyncratic rendering of the task of game theory (but see Aumann 1985).

4. A skeptic might entirely discount the prospects for the sort of conversation I propose. On such a view, theorists are so psychologically wedded to the standard explanatory mechanisms of their discipline that (under ordinary circumstances, at least) they are incapable of recognizing the plausibility of rival mechanisms (Stinchcombe 1991, 374–75). I obviously cannot promise that the conversation I propose will actually occur. But insofar as both critical theory and game theory suffer from the theoretical problems I identify, advocates of each have good reason to pursue it. Precisely because their circumstances are not "ordinary," conversation between them is possible.

5. For criticisms of this distinction as Habermas draws it, see Johnson 1991a. For some recent clarifications see Habermas 1991.

6. Habermas insists that communicative action derives its force from rational agreement or consent and *not* from either normative consensus or strategic compromise (1984, 296–97).

7. It is important to note that Habermas insists that speech acts derive force not just from *semantic* aspects of sentences but from *pragmatic* dimensions of utterances. Thus, a theory of meaning is inadequate to the task he sets himself. To this end, he advocates a formal pragmatics that can reconstruct the universal conditions of valid speech acts. This crucial point is beyond my present scope, however. See Habermas 1979, 1–68; idem 1992, 57–87.

8. See Habermas 1990, 80–81, 95–96, 116, 129–30. But Habermas explicitly recognizes that his argument from performative contradiction is not decisive (1990, 81, 95). It demonstrates that language use is not without presuppositions. However, it does not sustain any particular characterization of its presuppositions.

9. See Tugendhat 1985; White 1988, 45–46; and Wood 1985. Habermas seeks to defuse these criticisms by making certain "terminological clarifications" (1991, 239–45). These refinements do not bear directly on the present issues. It is important only to note that Habermas himself insists that his clarifications do not alter his basic distinction between communicative and strategic action (but see Bohman 1988).

10. Habermas inadvertently creates the impression that his analysis is such an argument in the following passage: "If we were not in a position to refer to the model of speech we could not even begin to analyze what it means for two subjects to come to an understanding with one another. . . . Naturally, speech and understanding are not related to one another as means to end. But we can explain the concept of reaching understanding only if we specify what it means to use sentences with communicative intent. The concepts of speech and understanding reciprocally interpret one another" (Habermas 1984, 287). To "use sentences with communicative intent," however, is precisely to extend a warranty to redeem, if necessary, the validity claims that your utterances raise.

11. For Habermas, "competences" are universal "capacities to solve particular types of empirical–analytic or moral–practical problems" (1990, 33). On the category of "interactive competence" see Habermas 1979, 73.

12. On the distinction between parametric and strategic rationality, see Elster 1979, 18–19, 117–18. Although he is inconsistent on this point, Habermas at times seems to

subscribe to something like the relatively benign view of strategic action presented here (1984, 85, 172–74). See Johnson 1991a.

13. I am indebted to Michael Neblo for bringing this passage to my attention during a conversation in May 1991 and for more generally helping to clarify my thoughts on the subject of strategic competence.

14. Game theorists sometimes acknowledge that coordination generates power (Hardin 1990, 363, 367–69; Schelling 1960, 108). Critical theorists would be concerned to question the legitimacy of resulting outcomes. Recognizing the role of communication in strategic interaction might afford them an opening from which to do so.

15. See Schelling (1978, 225–26). Inefficient equilibria are a species of collective irrationality. They are stable situations where all relevant actors proceed in such a way that all could be better off if all acted differently. The classic example is mutual defection in the one-shot prisoner's dilemma. Critical theorists, however, should note that the suboptimal outcome in a prisoner's dilemma results less from strategic rationality than from the nongeneralizable interests that subvert it. In a prisoner's dilemma, it does not matter what others will do—defection is a dominant strategy. It is in each player's interest to defect *irrespective* of the anticipated actions of others. And this, on the standard story, is because the prisoners are prevented from communicating binding commitments.

16. See Shubik 1982, 7–8 and Crawford 1990, 214, 216, 218. In game theory, a player's "strategy" must allow for every possible move on the part of other players (Rapoport 1966, 44; Shubik 1982, 34). Binmore (1990, 117–20, 176–78) and Rubinstein (1991) explore problems with this definition.

17. Habermas casts *The Theory of Communicative Action* largely as "a history of theory with systematic intent" (1984, 139–40). Toward that end, he engages in "the flexible exploration and deliberate exploitation of important theories constructed for explanatory purposes." My account in this section can be read in similar terms. For a more detailed, orthodox account see Aumann 1989.

18. See Harsanyi 1977a, 84; Schelling 1978, 17; and Myerson 1992, 66–69. This can, but need not, mean that actors are motivated by narrow self-interest (Elster 1979, 116; Shubik 1982, 81).

19. Compare Binmore 1990, 20.

20. The basic equilibrium concept in game theory is a Nash equilibrium, named for John Nash, who in 1950 proved formally that under certain conditions (specifically, that actors are allowed to play "mixed" strategies) there is at least one equilibrium in any finite n-person game (Shubik 1982, 242). It goes without saying that the existence of an equilibrium outcome does not entail that it is or can be normatively justified (Schelling 1978, 25–27).

21. See also Shubik 1982, 242–47. Elster (1986) discusses the implications of this point for rational choice theory more broadly.

22. Kreps is a nontechnical sketch of this result (1990b, 505–15). Fudenberg and Maskin (1986) is a technical presentation.

23. In political science, by contrast, formal theorists attempting to model elections and voting procedures have redescribed the "game" by introducing structure of various sorts into their models. See, e.g., Shepsle (1989) on the role of institutional arrangements and Elster (1989) on the effects of social norms. For a persuasive critical account of these efforts, see Knight 1992.

24. "Equilibrium refinements are strengthenings of the requirement that behavior constitute a Nash equilibrium, usually strengthenings that invoke in some manner or other the idea that players should not be allowed to make incredible threats or promises or to draw incredible inferences from things they observe" (Kreps 1990a, 108). In the passage cited earlier, Harsanyi parenthetically mentions one example—a "perfect" equilibrium—of this sort of refinement.

25. There are two reasons why one might resist this reading. The first is a more or less ideological faith that even in areas of social and political life not governed by markets, uncoordinated individual action should generate equilibrium outcomes similar to those generated by markets. There is little reason, however, to treat unique, market-induced equilibria as more than a special case (Schelling 1978, 25–26). The second reason consists of a commitment to "positive science." On such a view, an equilibrium provides the basis for testable predictions (Harsanyi 1977b, 4; Ordeshook 1986, 98). Conversely, multiple equilibria compromise testability, thereby threatening game theory's scientific status. But this is a too narrow and, while common, not an especially compelling, rendering of game theory. See Johnson 1991b and Bohman 1991.

26. Critical theorists might resist my use of the word *mechanism* here, but this is precisely the term Habermas uses to depict the force of speech acts in coordinating social and political interaction (e.g., 1984, 101).

27. For a nontechnical survey of how communication figures in game-theoretic models of political decision making see Austen-Smith 1992.

28. The formal results are especially important for two reasons. First, experimental studies have difficulty discriminating between the possible mechanisms underlying discussion-induced cooperation (Cooper et al. 1990, 230; Orbell, Van de Kragt, and Dawes 1988). Second, the formal results suggest that speech acts per se have force as a mechanism for coordinating social interaction. They thus challenge a tacit, if common, premise of experimental studies that view discussion as important only insofar as it sustains some other factor (e.g., generalized norms, contract-based promising, social identity), which in turn coordinates interaction. See, e.g., Orbell, Van de Kragt, and Dawes 1988. While it may be plausible to see rudimentary norms or identities arising in face-to-face discussion among experimental subjects, it is difficult to imagine how they could emerge among the strategic actors who populate formal game-theoretic models.

29. Elster provides a nontechnical review of game-theoretic models of bargaining (1989, 50–96). Coordination problems are endemic to bargaining models regardless of whether they represent negotiations as a cooperative or noncooperative game (Crawford 1990).

30. This may seem to be a harsh judgement. Consider, however, a related comment by Ariel Rubinstein, who, after briefly considering recent attempts to incorporate cheap talk into game-theoretic models, concludes, "It is my impression that although language plays a crucial role in resolving conflicts, game theory has so far been unable to capture this role" (1991, 921). While this is surely true at present, we can remain agnostic about whether it is true in principle.

31. Critical theorists will not find this terribly surprising. They should recognize, however, that the distorting factor here is nongeneralizable interest and not, as they tend to presume, strategic rationality itself. This surely suggests that rather than decrying the allegedly baleful impact of strategic rationality, critical theory ought to explore how nongeneralizable interests disrupt *both* communicative and strategic interaction.

32. Recent developments more or less directly build upon this notion of neologism-proofness. Matthews, Okuno-Fujiwara, and Postlewaite (1991), for instance, explore various extensions in the context of face-to-face interactions. Myerson, by contrast, introduces institutional structure in the guise of a mediator who, by randomizing over messages, can induce players to adopt "correlated" strategies (1989; idem 1991, 249–58, 283–88). In Myerson's work social interactions are coordinated less by the force of language per se than by the institutionalized capacities of the mediator. Since I am here concerned with the force of language in "institutionally unbound" interactions, this development, while interesting in its own right, is strictly speaking beside the point.

33. Recall that according to Habermas, this process can operate implicitly in everyday communicative practice or explicitly and reflectively in discourse or argumentation.

34. In such interactions, self-interest gives each player an incentive to adopt "dramatically anticommunicative" ran-

domizing strategies precisely for the purpose "of expunging from the game all details except the mathematical structure of the payoff, and from players all communicative relations" (Schelling 1960, 105, 165).

35. The relevant actors might revert to strategic interaction and risk indeterminacy, or they might break off relations altogether (Habermas 1984, 17–18; idem 1979, 3–4).

36. For an interesting move in this direction, see Bohman 1988.

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mes Johnson is Assistant Professor of Political Science, University of Rochester, ochester, NY 14627.

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THE SUPREME COURT AS A COUNTERMAJORITARIAN INSTITUTION? THE IMPACT OF PUBLIC OPINION ON SUPREME COURT DECISIONS

WILLIAM MISHLER *University of South Carolina*
REGINALD S. SHEEHAN *Michigan State University*

Although normative questions about the role of the Supreme Court as a countermajoritarian institution have long excited controversy in democratic theory, empirical questions about how far the Court acts contrary to majoritarian opinion have received less attention. Time series analyses for the period 1956–89 indicate the existence of a reciprocal and positive relationship between long-term trends in aggregate public opinion and the Court's collective decisions. The Court's ideological composition changes in response to previous shifts in the partisan and ideological orientation of the president and Congress. The Court also responds to public opinion at the margins even in the absence of membership change. Since 1981, the relationship has vanished or turned negative in direction. The Court's ideological balance has been upset by an unbroken string of conservative-to-moderate appointments, thereby undermining the dynamics that promote judicial responsiveness and raising questions about the majoritarianism of the contemporary and future Court.

> No matter whether th' constitution follows th' flag or not, th' supreme coort follows th' ilicition returns.
> —(Mr. Dooley [Finley Peter Dunne])

The role of the Supreme Court in national policymaking has long excited controversy in American democratic theory. Little argument exists about the role played by the Court; constitutional and judicial scholars agree that the Court has considerable policymaking influence. The controversy, rather, surrounds the seeming incongruity of an unelected and therefore "undemocratic" institution exercising significant political power in a democratic society. Bickel (1962) refers to this as the "countermajoritarian difficulty" of the Court. He argues that declaring actions unconstitutional that were initiated by duly elected representatives vitiates the will of the people. Although Bickel refers specifically to the power of judicial review, his critique can be extended to the exercise of other judicial powers in the policymaking process as well.

The rationale traditionally advanced for investing substantial political power in an unelected Court is the protection of minorities from democratic excess. The line between popular sovereignty and majority tyranny has proven notoriously difficult to draw. Nevertheless the assumption has been that the Court functions, at least to a degree, independent of majority opinion or the popular mood.

Although the traditional view of the Court as a countermajoritarian institution has always been controversial on normative grounds, the empirical validity of this perspective is subject to debate, as well. In a celebrated series of articles beginning with Dahl's (1957) classic essay, judicial scholars have engaged in a continuing and increasingly rigorous debate about the extent to which the Court is, in fact, insulated from majoritarian pressures.

We here join that debate. Specifically, we use time series methods and aggregate data to examine the relationship between public opinion and the decisions of the Supreme Court during the period 1956–89. In addition to measuring the strength of the relationship between public opinion and the Court's decisions and attempting to assess its causal direction, we develop and test alternative hypotheses about the pathways of public influence on the Court. Of particular interest is whether the Court attends directly to shifts in public opinion or responds only indirectly to shifting tides of public opinion through politically motivated changes in the membership of the Court.

PUBLIC OPINION AND THE SUPREME COURT

Among the first to challenge the empirical validity of the traditional view of the Supreme Court as a countermajoritarian institution, Dahl argued, "The view of the Court as protector of the liberties of minorities against the tyranny of majorities is beset with . . . difficulties that are not so much ideological as matters of fact and logic" (1957, 283). Observing that the Court only rarely overturned recently enacted policies of extant law-making majorities in Congress and the White House and almost never to protect the interests of identifiable minorities, Dahl concluded that American politics is characterized by relatively stable and enduring electoral/political coalitions that dominate national policymaking. Moreover, "Except for short-lived transitional periods when the old alliance is disintegrating and the new one struggling to take control of political institutions, the Supreme Court is inevitably a part of the dominant national alliance. As an element in the political leadership the Court inevitably supports the major policies of the

p. 293). Dahl's thesis, of course, is a much ı and sophisticated version of the conven- dom succinctly expressed by Mr. Dooley in ing epigram. Nevertheless, the Dahl-Dooley sis, as Funston (1975) labeled it, has its critics. rhaps the most widely cited critique of this ctive, Casper (1976) argued that Dahl substan- underestimated the extent to which the Court s political influence independently of, and fre- tly in opposition to, law-making majorities. Crit- ng the exclusive focus on cases in which the urt held federal legislation unconstitutional, sper expanded Dahl's analysis to include state and cal legislation nullified by the Court, as well as ases where the Court altered national policy by nterpreting federal statutes creatively.[1] Based on this additional data, he concluded that the Court was substantially more active in protecting minorities—and more willing to alter or reinterpret the policies of the dominant law-making coalition—than Dahl's earlier analysis allowed.

Although the relationship of the Court's decisions to public opinion is central to this debate, none of the authors addresses that issue directly. Lacking direct evidence on public opinion because "scientific opinion polls are of relatively recent origin," Dahl used the *position of the legislative majority in Congress* as a surrogate for majority will (1957, 283). If the Court overturned a law recently enacted by Congress, he considered it to have countermanded the will of the dominant coalition and thus, indirectly, of majority public opinion. Funston (1975) and Casper (1976) used similar strategies.

Aided by the increasing availability of social surveys and public opinion polls, several recent studies (e.g., Barnum 1985; Page and Shapiro 1983) have examined the relationship between public opinion and the Court's decisions more directly, exploring the congruence between the Court's decisions in specific cases and public attitudes toward the central issues in those cases. In the most ambitious study of this type, Marshall (1989) compares specific Supreme Court rulings with national public opinion polls from the mid-1930s through the mid-1980s. He observes that "the modern Court has been an essentially majoritarian institution. Where clear polls margins exist, three-fifths to two-thirds of Court rulings reflect the polls" (p. 192). Marshall finds that the level of congruence varies by policy area but concludes that the Supreme Court, overall, was about as responsive to public opinion as were other policymaking institutions. (For a contrasting perspective based on a very different style of analysis, see G. Rosenberg 1991.)

Although generally supportive of the Dahl–Dooley hypothesis, congruence studies have their limitations as well. For one, congruence tells us very little about causal direction (Caldeira 1991). It cannot distinguish situations where the Court responds to public opinion from those where public opinion is shaped by the decisions of the Court.[2]

Compounding this problem is the tendency of public opinion studies to focus on the decisions of the Court only in a few landmark cases. Such cases are of obvious interest because of their political importance. Precisely for this reason, however, they are likely to be the cases in which the public is most interested and public opinion most intense. They also are likely to be the cases for which public opinion is most salient. As a result, these studies are limited in what they can tell us about the relationship of broader trends in public opinion and the myriad, more typical cases decided by the Court with little fanfare or publicity.

Another limitation of existing opinion studies identified by Barnum is that the questions asked in public opinion polls frequently do not correspond very closely with the specific issues in the cases with which they are matched (1985, 654). Of necessity, these studies must match specific decisions with broad opinions (e.g., comparing the limited right to abortion provided in *Roe* v. *Wade* with public responses to broad questions about support for abortion or even about the rights of woman).

The small number of Supreme Court cases in any issue area combined with the absence of extended times series data on public opinion in most policy areas also limits existing studies to relatively simple comparisons of the outcome of a single case with the *level or direction of public opinion at the moment.* This limits the introduction of statistical controls and prevents examination of the possibility that the impact of public opinion on the Court may occur at a significant lag.

More importantly, the focus on individual decisions and public opinion on specific policy issues at the moment assumes, in effect, that the impact of public opinion is direct and immediate—that the decisions of justices on pending cases are influenced by relevant public opinion polls seen in the morning paper. A more realistic assumption is that public opinion, if it is important, influences the Court as a result of gradual, almost imperceptible changes in the attitudes and beliefs of individual justices as they adapt, consciously or not, to long-term, fundamental trends in the ideological temper of the public. Examination of the later possibility requires extended time series data, previously unavailable, on ideological trends both in public opinion and Supreme Court decisions.

In short, the nature and extent of the relationship between public opinion and Supreme Court decisions remains very much in question.

PATHWAYS OF INFLUENCE

As to the processes or mechanisms by which public opinion might influence the Supreme Court, Dahl attributed the Court's reluctance to invalidate majoritarian policies to the president's power of judicial appointment (1957, 284). He argued that it would be "naive to assume that the Supreme Court either would or could play the role of Galahad," modifying its positions to accommodate public opinion. Reasoning that the average president in American history served for two terms and appointed two justices to the Court per term, he concluded that the Court was responsive because most presidents enjoyed ample opportunity "to tip the balance on a normally divided Court."

Elaborating this thesis, Funston (1975) demonstrated that the periods when the Court has been out of step with the dominant political coalition coincide with electoral realignments. In his view, the fact that these periods have been brief reflects the speed with which the dominant coalitions that emerge from realignments are able to appoint enough justices to alter the decision-making balance on the Court. Although Funston's data have been criticized (Canon and Ulmer 1976), the Dahl–Funston hypothesis regarding the primacy of membership change is widely accepted (see, e.g., Baum 1992; Schubert 1970; Segal 1985).

Notwithstanding the prominence of the Dahl–Funston hypothesis, another perspective holds that the Court can and does respond to public opinion even in the absence of membership change. The argument usually advanced is that Supreme Court justices are acutely aware of the limitations of the Court's power and its dependence on voluntary acquiescence to its decisions (see, e.g., Adamany and Grossman 1983). As Justice Frankfurter expresses it in *Baker* v. *Carr* (1962), "The Court's authority—possessed of neither the purse nor the sword—ultimately rests on sustained public confidence in its moral sanction."

According to this argument, the Court's concern for its authority makes it reluctant to depart too far or too long in its decisions from prevailing public sentiment. This is not to say that judges "play the role of Galahad." Neither is it to argue that justices consult opinion polls prior to making decisions or that they tailor their judicial philosophies to the latest fluctuations in public opinion on specific issues. It is rather to suggest both that members of the Court are political creatures, who are broadly aware of fundamental trends in ideological tenor of public opinion, and that at least some justices, consciously or not, may adjust their decisions at the margins to accommodate such fundamental trends.[3]

Even in the absence of concerns about the legitimacy of the Court's decisions, there are good reasons to believe that Supreme Court justices may be influenced by long-term changes in the climate of public opinion. Justices are no less susceptible than other individuals in society to influence by evolving societal norms and values. If, for example, attitudes in the political culture toward the role of women in society undergo significant changes over time, it is unlikely that the attitudes and beliefs of sitting justices can remain permanently immune to these changing mores. Gradually, the attitudes at least of some of the justices are likely to change, thus shifting the Court's ideological center of gravity, as well.

The likelihood, of course, is that under either of these variants of what we shall call the *political adjustment hypothesis*, the resulting changes in the decisions of individual justices will occur gradually over time and be almost imperceptible at the moment. Thus, it is unrealistic to expect the results of a specific public opinion poll to be linked to specific decisions either of an individual justice or of the Court as a whole. The expectation, rather, is that the overall pattern of justices' decisions—and thus the ideological center of gravity of the Court—should respond gradually, and at a considerable lag, to broad trends in the ideology of the public.

Little systematic evidence exists with respect to the political adjustment hypothesis, although anecdotes abound. A classic example, frequently cited, was the apparent retreat by the Supreme Court in the 1930s from its initial opposition to President Roosevelt's New Deal legislation (see, e.g., the citations in Caldeira 1987). Confronted by political pressures and by hostile public opinion mobilized in part by FDR in support of his "Court-packing" scheme, the Court abruptly reversed its string of anti–New Deal decisions upholding minimum-wage laws in *West Coast Hotel* v. *Parrish* (1937) and the Wagner Act in *NLRB* v. *Jones and Laughlin Steel Company* (1937), among other decisions. It did so, moreover, without any change in membership.

Also frequently cited is the Court's decision in *Koramatsu* v. *the United States* (1944), in which the popularly supported and presidentially ordered internment of Japanese-Americans was upheld by the Court despite widespread misgivings by constitutional scholars and jurists about the constitutionality of this decision. Even Chief Justice Rehnquist has acknowledged that the Court's decision in *Youngstown Sheet and Tube Company* v. *Sawyer* (1952) was probably influenced by popular sentiment toward President Truman and the Korean War.[4] Indeed, Rehnquist has argued:

> Judges, so long as they are relatively normal human beings, can no more escape being influenced by public opinion in the long run than can people working at other jobs. And, if a judge on coming to the bench were to decide to hermetically seal himself off from all manifestations of public opinion, he would accomplish very little; he would not be influenced by current public opinion, but instead would be influenced by the state of public opinion at the time he came to the bench (1986, 40–41).

Still further indicative of the importance of public opinion for the Court is the evidence that members of the Court may have at times attempted to manipulate public opinion. In an ingenious study drawing upon a series of early Gallup polls tracking public opinion across the period of the Court-packing fight, Caldeira (1987) demonstrates that the Court's precipitous change of direction on New Deal legislation played a critical role in undermining public support for FDR's Court-packing plan. Caldeira refrained from attributing political motives to the Court based on this evidence. However, his data, together with the anecdotes about other episodes in the Court's history, certainly suggest the plausibility of the political adjustment hypothesis.

METHODS AND MEASURES

To examine the relationship between public opinion and Supreme Court decisions, we use aggregate yearly data for the period 1956–89. Data on Supreme Court decisions are available from the U.S. Supreme Court data base (Spaeth 1991), which includes all Supreme Court decisions during the period of study.

Given both the relatively small number of cases for many specific policy areas in a number of the years under study and our concern with measuring broad ideological trends, we decided not to attempt the development of policy-specific measures of the Court's decisions.[5] Rather, we developed a composite measure of the overall ideological tenor of the Court's decisions each year. The Supreme Court data base codes each of the nearly seven thousand cases decided during this period according to whether its outcome was liberal or conservative in direction.[6] The liberalism of Court decisions in any year is then calculated simply as the percentage of all cases in a year decided in a liberal direction.[7]

In developing an index of the liberalism of Court decisions, we considered only the approximately four thousand cases that were decided by full opinion and oral argument. *Per curiam* cases and cases decided by memorandum were excluded because of concerns that many such cases are highly routine. Such cases are unlikely either to influence, or to be influenced by, public opinion.[8]

To measure the ideological main currents of public opinion across this 33-year period, we relied on Stimson's (1992) innovative index of the liberalism of what he calls the "public mood" in America.[9] Stimson identified 139 different domestic social and economic policy questions that had been asked in identical form in major public opinion polls in two or more years between 1956 and 1989. After determining the liberal–conservative direction of public responses to each question, he constructed a composite measure from the individual items. The resulting index measures the relative liberalism–conservatism of the public mood across a broad range of domestic policy concerns on a yearly basis for the period 1956–89.[10]

To consider the Dahl–Funston hypothesis that linkages between public opinion and Supreme Court decisions are mediated through the changing ideological composition of the Court, we again turned to existing measures. Specifically we used the ideology scores for individual justices constructed by Segal and Cover (1989) based on analyses of the justices' judicial philosophies at the time of their confirmation hearings.[11] To create an aggregate, yearly measure of the ideological composition of the Court, we summed Segal and Cover's individual ideology scores across the nine justices sitting on the Court in any year. A useful feature of this index is that it identifies the ideology of the justices based on their behavior before their respective appointments to Court, rather than on their voting records while on the Court (Sheehan, Mishler, and Songer 1992). This avoids the problem of circularity inherent in studies that attempt to explain Supreme Court decisions using measures based on those same decisions.

Finally, several additional measures were constructed to control for the possibility that the effects of public opinion on the Court are mediated through the effects of the president and Congress. Two aspects of the presidency were examined: the party of the president and a partisan-controlled measure of presidential popularity.[12] Two measures of the decision-making majority in Congress were also considered: the simple percentage of seats in the House of Representatives controlled by Democrats and a composite measure reflecting the presence and success of the conservative coalition in the House of Representatives.[13]

AN OVERVIEW OF BASIC TRENDS

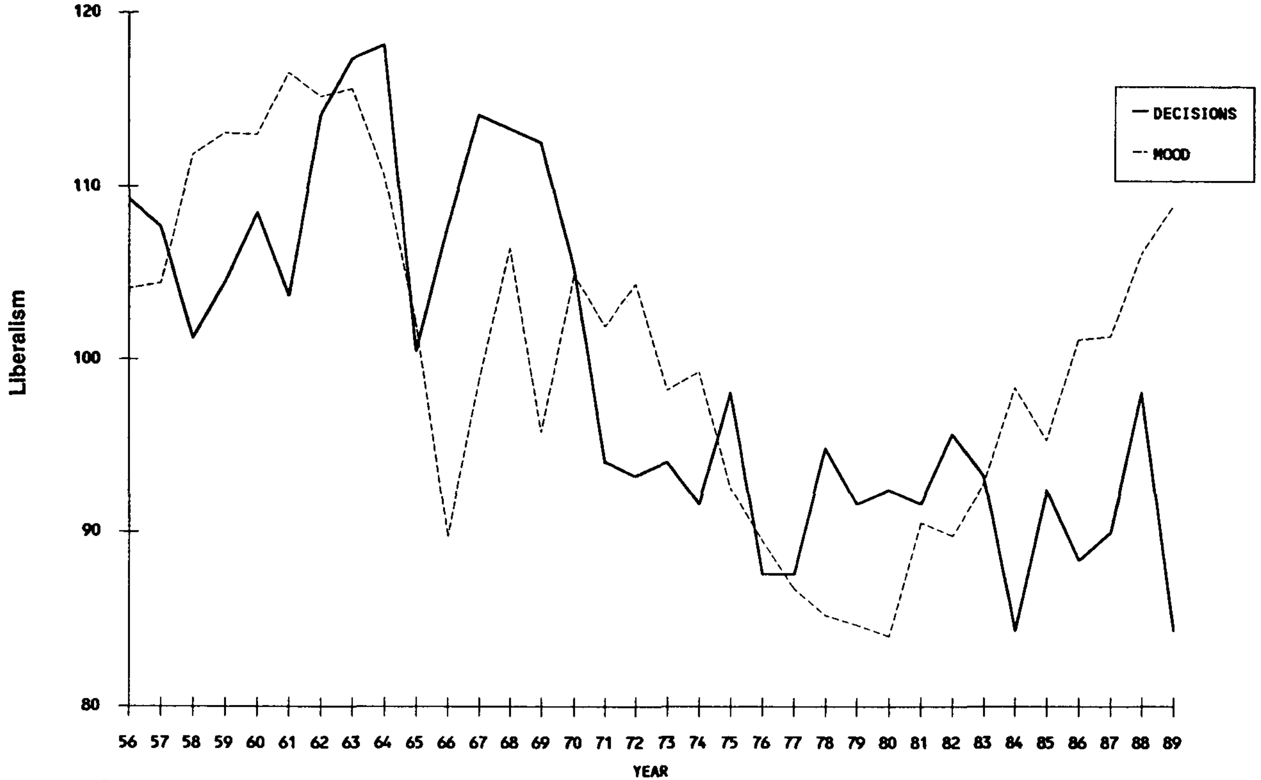
Figure 1 compares the liberalism of the public mood and Supreme Court decisions for the period 1956–89.[14] Indicative of the face validity of our measure of Supreme Court decisions, the trends in the liberalism of the Court's decisions accord very well with what we think we know about recent history. Our measure indicates that the Court's decisions become increasingly liberal during the early Warren years, averaging a full standard deviation above the 33-year mean throughout the 1950s and early 1960s. The liberalism of the Court levels off in the mid-1960s, then falls precipitously across the Burger years, after which there is a continuing, but much more gradual and intermittent, increase in conservative decisions during the 1980s.

Also noteworthy in Figure 1 is the close correspondence between the trends in the Supreme Court decisions and the liberalism of the public mood across most of the period. Both series register increasing levels of liberalism during the 1950s and early 60s; both decline abruptly during the latter 1960s; and both reach bottom circa 1980, although they clearly diverge thereafter. Nevertheless, the simple correlation between the series (r = .56), though statistically significant, indicates that there are significant discrepancies in these trends, as well.

Part of the reason the correlation between the public mood and the Court's decisions is not stronger is that the strength and timing of their common trends clearly differ. These differences provide clues to the direction of causality. Figure 1 suggests that changes in the public mood generally precede the associated changes in the liberalism of Supreme Court decisions. For example, the increasing liberalism evident in both series in the late 1950s and early 1960s begins somewhat earlier in the public mood, as does the subsequent conservative tide observed in both series in the late 1960s and early 1970s. Al-

FIGURE 1

Standardized Measures of the Liberalism of the Public Mood and the Liberalism of Supreme Court Decisions



though the conservatism of the Court overtakes public opinion during the Nixon years, public opinion maintains its gradually conservative trend throughout the 1970s, anticipating by several years the increased conservatism of the Court during the Reagan years. The principal exception to this pattern occurs in the period since 1980, in which the resurgence of liberalism in the public mood is unmatched by any similar trend in the Court's decisions.

Consistent with the emphasis accorded membership change in explaining changes in Supreme Court decisions, Figure 2 demonstrates even greater congruence between the ideological composition of the Court and trends in the liberalism of the Court's decisions (r = .77). The strength of this relationship is consistent with the Dahl–Funston hypothesis of the centrality of membership change in explaining fluctuations in the Court's decisions. Nevertheless, although closely aligned, there are indications of relatively short lags in this relationship, as well. Particularly notable is the evidence that the decline in the liberalism of the Court's decisions in the late 1960s began somewhat *before* the first of President Nixon's conservative appointments to the Court. This evidence, however preliminary and tentative, accords with the political adjustment hypothesis that the Court can and does respond to political trends even in the absence of a change in its membership.

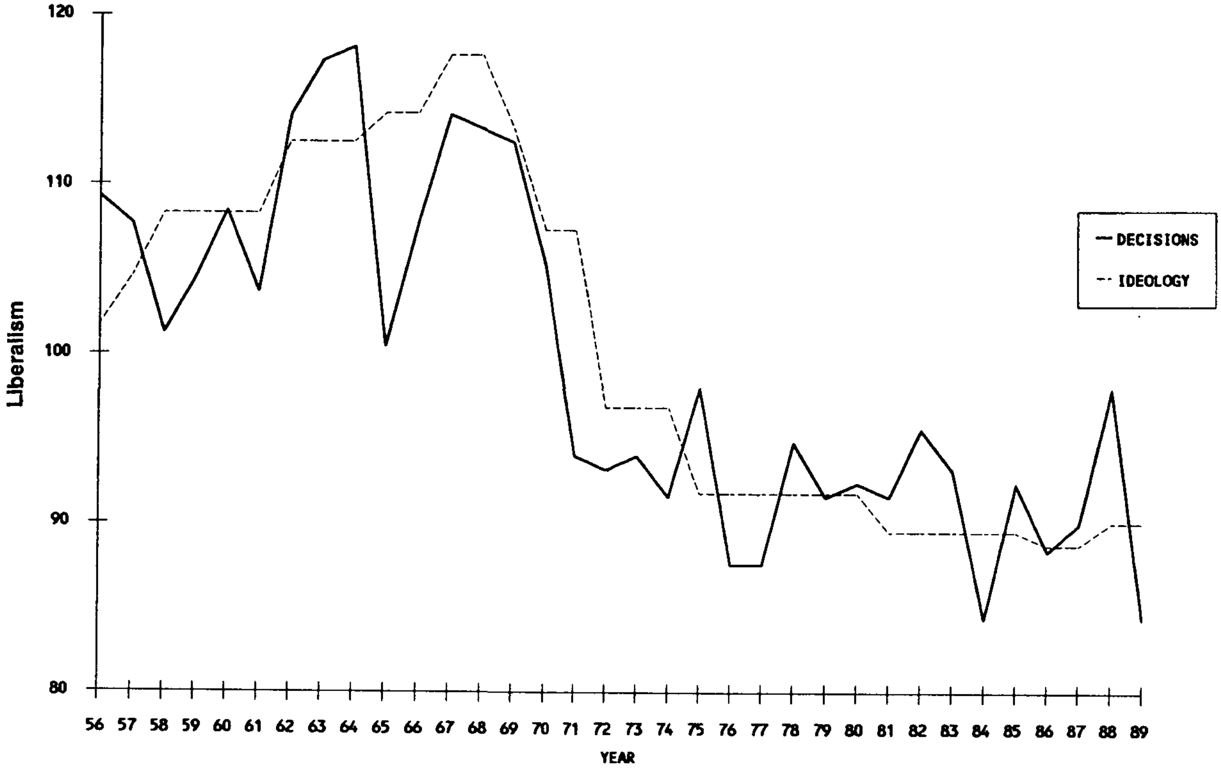
LINKAGES AND LAGS

Simple inspection of trend data is useful for obtaining a broad overview of the relationships between public opinion and Supreme Court decisions; but greater rigor is required to estimate the nature and extent of this relationship, determine its timing or lag structure, establish causality, and assess the viability of alternative pathways of influence. Moreover, care must be taken to control for the high levels of autocorrelation found in several of the variables.

Existing theory provides little guide to specifying the lags that might exist in the relationship between the ideological tenor of the public mood and the Court's decisions. Under either of the hypothesized pathways of influence, a significant lag is expected. According to the Dahl–Funston hypothesis, it takes time for a shift in public opinion to be reflected in presidential politics; and additional time is needed for a newly elected president to have opportunities to fill vacancies on the Court. Even where the Court responds directly to perceived changes in opinion as proposed by the political adjustment hypothesis, it still takes time for justices to perceive a change in the public mood. More importantly, it takes time for societal trends to be absorbed and reflected in the attitudes and beliefs of justices or for justices to

FIGURE 2

Standardized Measures of the Ideological Composition of Supreme Court Membership and the Liberalism of Supreme Court Decisions



become convinced that a change is of sufficient salience, magnitude, and endurance as to threaten the Court's authority.

Lacking more specific theoretical guidance, we proceeded empirically to identify the lengths of possible lags. Using Box–Jenkins time-series procedures (Box and Jenkins 1976; Box and Tiao 1975), we identified the autocorrelation in the public opinion series as a simple, first-order autoregressive, or AR(1) process. An AR(1) model then was used to filter the measures of public opinion and the Court's decisions, and the filtered measures were cross-correlated at lags of 0–10 years. Although principally interested in the impact of public opinion on Supreme Court decisions, we examined lags in both directions because of the possibility that the relationship would be reciprocal.

The cross correlations are reported in Table 1. The coefficients in the lower half of the series indicate the correlations between public opinion at time t and the Court's decisions in later years t + 1 to t + 5. Logically, these indicate the potential impact of public opinion on subsequent Court decisions. The correlations in the top half of the series measure relationships between opinion at t and decisions in earlier years t − 1 to t − 5. These reflect possible reciprocal effects of the Court's decisions on public opinion.[15]

As expected, removing the autocorrelation in these series substantially reduces the zero-order correlation (the middle coefficient); the resulting coefficient is small and statistically insignificant. Nevertheless, the public mood is significantly and positively correlated with trends in the Court's decisions at a lag of five years; and the relationship approaches significance at lag t + 3, as well.

The strong positive relationship between the Court's decisions and the public mood at lag t − 1 also supports the suggestion that the relationship is reciprocal. Although a substantial literature documents the impact of specific Court decisions on public opinion (frequently in a negative direction), the very preliminary evidence here suggests that the broad pattern of Court decisions has a generally positive impact on the public mood. More generally still, this indication of reciprocal links between the public mood and the Court's decisions hints at the existence of a relationship in which (1) the ideological tenor of the Court's decisions are responsive in the long run to the shifts in the ideology of the public mood and (2) the Court's decisions subsequently reinforce and legitimize those shifts in mood. This is highly speculative, of course. Here, we shall concentrate on the possible impact of the public mood on the decisions of the Court and leave the reciprocal relationship to a later study.

As a preliminary step in examining how far linkages between public opinion and the Court's deci-

TABLE 1

Cross Correlations between the Liberalism of Supreme Court Decisions and the Ideological or Partisan Composition of the Public Mood, Presidency, Congress, and Supreme Court

COURT DECISIONS[a]	PUBLIC MOOD[b]	DEMOCRATIC PRESIDENT'S POPULARITY[c]	DEMOCRATIC PRESIDENT[d]	% SEATS DEMOCRATIC[e]	CONSERVATIVE COALITION[f]	IDEOLOGICAL COMPOSITION OF COURT[g]
t − 5	−.07	−.17	.12	−.13	.19	.17
t − 4	.18	−.13	.23	.01	−.20	.30*
t − 3	−.12	.13	−.17	.04	−.08	.07
t − 2	−.14	.18	.10	.25	−.09	.31*
t − 1	.40*	.00	.00	−.20	.15	.37*
t	−.01	.02	.04	−.10	−.20	.28
t + 1	.12	.08	.33*	−.12	.19	.44*
t + 2	.08	.24	.24	.07	−.11	.22
t + 3	.26	.34*	.17	.25	.10	.22
t + 4	.19	.02	−.22	.23	−.30*	.20
t + 5	.36*	.11	.19	−.02	−.14	.28

Note: All variables are filtered by a first-order autoregressive model, $(1 - .73B)y_t = a_t$, prior to analysis.
[a] Yearly percentage of Supreme Court cases decided in a liberal direction.
[b] Index of the liberalism of public opinion on domestic policy concerns.
[c] Partisan-controlled index of presidential approval (see n. 18).
[d] Dummy variable scored 1 if a Democrat is president, 0 otherwise.
[e] Percentage of Democratic seats in the House of Representaitves.
[f] Percentage of roll call votes in which a majority of Republicans and southern Democrats opposed and defeated a majority of northern Democrats in House of Representatives.
[g] Sum of liberalism scores of Supreme Court justices at time of initial appointments.
*$p \leq .05$.

sions are mediated through elite opinion or the changing composition of the Court, Table 1 also reports the cross correlations between the Court's decisions and several measures of the ideological and partisan composition of Congress, the presidency, and the Supreme Court. Both the party of the president and a partisan-controlled measure of the president's popularity are moderately correlated with the Court's decisions—party at a lag of one year and popularity at a lag of three years. In contrast, the percentage of seats in the House of Representatives occupied by Democrats is unrelated to the Court's decisions at any lag up to 10 years; but the strength of the conservative coalition in the House is significantly and negatively correlated with the Court's decisions at a lag of four years. In none of these cases is there any evidence of reciprocal effects.

Finally, Table 1 also reports cross correlations between the liberalism of the Supreme Court's decisions and the ideological makeup of the Court's membership. As expected, the correlations are the strongest, and the lags the shortest, in the data. There also is clear evidence of reciprocal effects. The significant, positive correlation at lag t + 1 suggests that changes made in the composition of the Court in one year are reflected in the decisions of the Court beginning the next session. Both the relative strength of the correlation and its short lag are consistent with the Dahl–Funston hypothesis that the composition of the Court is the proximate cause of decision change and an important intervening link to other putative causes.

Equally interesting, however, is the evidence of reciprocal relationships. Significant correlations at lags t − 1, t − 2, and t − 4 suggest that changes in the membership of the Court are as likely to *follow* changes in the Court's decisions as they are to *precede* them. This is not to suggest that changes in the Court's decisions "cause" the membership of the Court to change. Our suspicion is, rather, that the correlations are spurious consequences of simultaneous "political adjustment processes" operating on the president and Congress, as well as the Court.

The political adjustment hypothesis holds that members of the Court, perceiving a fundamental shift in public opinion, adjust their decisions at the margins to accommodate that shift. Since the president and Congress are presumably even more sensitive to public opinion, the likelihood is that the same forces that prompt the Court to adjust its decisions will influence the president's judicial appointments and thus the future composition of the Court. In this sense, the Court's decisions not only are responsive to public opinion in the absence of membership change but appear even to anticipate changes in the Court's composition.

MULTIVARIATE MODELS

The lags identified in the cross-correlations were used to specify a series of multivariate models estimating the impact of public opinion on Supreme Court decisions (Table 2). The logic behind models A–D is roughly that of Simon–Blalock causal modeling (Blalock 1961) or what Morris Rosenberg (1968) calls the

TABLE 2

The Impact of Public Opinion on Supreme Court Decisions, 1956–1989

	MODELS			
	A[a]	B[a]	C[b]	D[b]
Intercept	−24.3 (30.7)	−1.98 (28.2)	−46.6* (23.9)	−11.95 (25.42)
Public Mood$_{t-5}$[c]	.77** (.30)	.49* (.27)	1.08** (.22)	.66** (.26)
Court Membership$_{t-1}$[d]	—	2.78** (.79)	—	1.76** (.70)
Democratic President$_{t-1}$[e]	—	—	5.41* (3.03)	5.52* (2.89)
Democratic President's Popularity$_{t-3}$[f]	—	—	−.31** (.12)	−.15 (.12)
Conservative Coalition$_{t-4}$[g]	—	—	−.86** (.34)	−.53 (.27)
Reagan Intercept[h]	140.9* (73.7)	90.3 (59.0)	161.3** (50.2)	114.2* (48.8)
Reagan × Mood$_{t-5}$	−1.59* (.81)	−.94 (.56)	−1.78** (.55)	−1.22* (.54)
AR(1)	−.52 (.18)	−.15 (.09)	—	—
R^2(*reg*)[i]	.35	.71	.80	.85
R^2(*tot*)[j]	.68	.78		
Durbin-Watson	1.86 (ns)	1.92 (ns)	2.03 (ns)	2.19 (ns)
Lung Box Q (df = 12)	10.6 (ns)	10.9 (ns)	11.8 (ns)	11.6 (ns)

Note: Standard errors are in parentheses. N = 34.
[a] Maximum Likelihood Estimates.
[b] Ordinary Least Squares Estimates.
[c] Index of the liberalism of public opinion on domestic policy concerns.
[d] Sum of liberalism scores of Supreme Court justices at time of initial appointments.
[e] Dummy variable scored 1 if a Democrat is president, 0 otherwise.
[f] Partisan-controlled index of presidential approval (see n. 18).
[g] Percentage of roll call votes in which a majority of Republicans and southern Democrats opposed and defeated a majority of northern Democrats in House of Representatives.
[h] Dummy variable coded 1 for years 1981 through 1989 and 0 otherwise.
[i] Variance explained by model excluding autoregressive parameter.
[j] Variance explained by model including autoregressive parameter.
*P ≤ .05 (one-tailed test).
**P ≤ .01 (one-tailed test).

process of elaborating relationships. We establish the existence of a relationship between public opinion and Court decisions (model A), then examine whether this relationship survives the introduction of controls for (1) the ideological composition of the Court (model B), (2) the ideological orientations of president and Congress (model C), and (3) all of the variables in combination (model D). Evidence that the relationship between public opinion and the Supreme Court vanishes when controls are introduced would validate the Dahl–Funston hypothesis that public opinion influences decisions of the Court only indirectly through the mediating influence of elections and the orientations of political elites on the ideological composition of the Court. Conversely, evidence that the effects of public opinion survive the introduction of these controls would support the political adjustment hypothesis that public opinion has significant direct effects on the Court's decisions independent of the composition of the Court or the political orientation of president and Congress.

We noted previously that the ideologies of the public mood and of the decisions of the Court have diverged rather sharply since President Reagan's election in 1980 (recall Figure 1). Public opinion grew increasingly liberal across this decade, whereas the Court's decisions remained relatively stable or became slightly more conservative. This departure from the overall correspondence of these series raises the possibility of parameter instability over time. A Chow test (not shown) comparing the residual variance of a preliminary version of model D (without the interactive terms for the Reagan years) for the time periods 1956–81 and 1981–89, was statistically significant, confirming the existence of significant differences in the model before and after 1981.[16] Thus, to account for differences in the impact of public opinion in the Reagan years, all of the models include an interactive term and associated slope coefficient for public opinion during the period 1981–89.

Model A estimates the impact of the public mood on the liberalism of Supreme Court decisions controlling only for the Reagan years and first-order autoregression.[17] It confirms the results of the cross correlations and indicates that the public mood "Granger-causes" Supreme Court decisions at a moderate lag.[18] The maximum likelihood coefficient is statistically significant at a lag of five years, and the model reduces the variance in the liberalism of the Court's decisions by 35% after adjusting for autocorrelation.

Also notable in model A is the coefficient for the interactive term measuring public opinion during the Reagan years. To determine the effect of the public mood during this period the coefficient for the interactive Reagan-Mood variable must be added to the coefficient for public mood more generally. Thus, in equation A the effect of the public mood during the Reagan years is:

$$Y = (-24.3 + 140.9) + (.77 - 1.59)X.$$

The impact of mood during the Reagan years is negative and about as large as the positive effect during the rest of the 1956–1989 period. That the coefficient for the interactive term is statistically significant indicates that the impact of mood differs significantly during the Reagan years compared to the rest of the period ($b = -.82$ vs .77).[19]

The discrepancy in the impact of public mood on the decisions of the Court before and after 1981 is equally strong in the other models, which control separately for the ideological composition of the Court (model B), the party and ideology of president and Congress (model C), and all of the variables simultaneously (model D). In each of these models, public opinion has strong, positive effects on the Court's decisions prior to 1981 but about equally strong and *negative* effects during the Reagan years.

Regarding the avenues by which public opinion registers on the Court, the evidence in models B and D clearly indicates that the impact of public mood on the Court's decisions survives the introduction of controls for the ideological composition of the Court and for the partisan and ideological orientation of Congress and president. This is the case both for the positive effects of public opinion prior to 1981 and the negative effects of public opinion thereafter.

That these effects are somewhat attenuated by the controls implies that the impact of public opinion on the Court's decisions is mediated partly through its effects on the changing membership of the Court. However, consistent with the political adjustment hypothesis, the persistence of strong public mood effects controlling for the membership of the Court indicates that the impact of public opinion on the Court's decisions is partially independent of membership change, as well.

A final observation in model D is the persistence of significant effects for the party of the president and the conservative coalition in Congress despite controls for the ideological composition of the Court. Here, again, the effects of president and Congress are reduced when the composition of the Court is controlled (compare models C and D), confirming, as expected, that the influence of president and Congress on the Court is mediated partly though the appointment and confirmation process. Again, however, the persistence of significant effects despite this control suggests that the Court responds to presidential and congressional sentiment in the absence of membership change and independently of public opinion, as well.

That the lags in these relationships are shorter for the party of the president and longer for the conservative coalition in Congress supports the expectation that the impact of the president on the Court is more immediate than that of Congress. Nevertheless, the consistency with which the Court's decisions appear to follow the partisan and ideological orientation of the president and Congress during the years prior to 1981 lends credence to Dahl's contention that the Court has traditionally acted as part of the dominant political coalition in America—at least prior to the Reagan years.

What emerges from these data, then, is an image of the Court prior to 1981 as an ideologically motivated but politically sensitive institution. The Court's decisions were influenced substantially by the ideologies of the sitting justices but responded independently to both public and elite opinion, as well. In the years since 1981, however, the Court appears as an even more ideologically driven but politically isolated institution. Its decisions have been independent of, if not antagonistic to, recent trends in the public mood and have been out of alignment with the dominant coalition in Congress. That the Court's decisions since 1981 have remained consistent with the partisan orientation of the president reflects, in all likelihood, the existence of divided government and the absence in the 1980s of any semblance of a dominant political alliance.

THE CHANGING COMPOSITION OF THE COURT

Notwithstanding the significant, independent effects of public and elite opinion on the Court's decisions prior to 1981, the changing ideological composition of the Court's membership clearly has been a principal dynamic underlying the Court's decisions both before and (especially) after 1981. To understand better the effects of public and elite opinion on the composition of the Court, a second set of models was constructed (Table 3).

Consistent with the Dahl–Funston hypothesis, the results indicate that changes in the party (though not the popularity) of the president and changes in the

TABLE 3

The Impact of Changes in Public Opinion on Year to Year Changes in the Ideological Composition of the Supreme Court, 1958–1989

	MODELS	
	A	B
Intercept	−.31*	−.66**
	(.15)	(.17)
Public Mood$_{t-6}$[a]	.09**	.05*
	(.02)	(.02)
Democratic President[b]		.81**
		(.23)
Democratic President's Popularity$_{t-1}$[c]	—	.00
		(.04)
Conservative Coalition$_{t-4}$[d]	—	−1.78**
		(.54)
Reagan Intercept[e]	.29	.63**
	(.28)	(.22)
Reagan × Mood$_{t-6}$	−.08*	−.01
	(.03)	(.03)
R^2	.39	.76
Durbin-Watson	2.21 (ns)	2.41 (ns)
Lung Box Q (df = 12)	8.11 (ns)	8.25 (ns)

Note: Standard errors are in parentheses. N = 34.
All estimates are ordinary least squares.
[a] Yearly change in the index of the liberalism of public opinion on domestic policy concerns.
[b] Dummy variable scored 1 if a Democrat is president, 0 otherwise.
[c] Yearly change in the partisan-controlled index of presidential approval (see n. 18).
[d] Yearly change in the percentage of roll call votes in which a majority of Republicans and southern Democrats opposed and defeated a majority of northern Democrats in House of Representatives.
[e] Dummy variable coded 1 for years 1981 through 1989 and 0 otherwise.
*P ≤ .05 (one-tailed test).
**P ≤ .01 (one-tailed test).

ideological composition of Congress significantly influence changes in the ideological composition of the Court.[20] Predictably, the party of the president appears to have a stronger impact on the composition of the Court.[21] Unlike the other variables in the model, the party variable was not differenced prior to analysis.[22] This means that the impact of party in the model increases steadily across the length of a president's term. The longer a party controls the White House, the more vacancies on the Court the president is likely to be able to make and the greater the impact the president can have on the Court's ideological orientation.

Unlike the previous model, the impact of public opinion on the ideological composition of the Court does not appear to vary appreciably across the two periods. Public opinion has a small but significant and positive impact on the composition of the Court across most of the time period. The negative coefficients for public mood during the Reagan years suggest a different relationship during this period, but the coefficients are too small to be of consequence.

Furthermore, the coefficients for public opinion in both periods are substantially attenuated by the introduction of controls for the party of the president and the ideological composition of Congress. Although the positive impact of public opinion on the Court's composition prior to 1981 remains significant after introducing the controls, its coefficient is reduced by almost half. This suggests that the effects of public opinion on the composition of the Court are limited and mediated substantially through the impact of public opinion on the party of the president and the composition of Congress.

DISCUSSION

The relationship of public opinion to the Supreme Court appears to be both subtle and complex. We cannot do justice to these subtleties here, nor do we claim to have done so. Instead, we have attempted in this analysis to provide a broad overview of the relationship and to begin the process of identifying directionality and causality.

Empirically, the results of our analysis are easily summarized. For most of the period since 1956, a reciprocal relationship appears to have existed between the ideology of the public mood in the United States and the broad ideological tenor of Supreme Court decisions. The impact of the public mood on the Court's decisions occurs at a moderate lag; a somewhat shorter lag appears to exist in the reciprocal effects of Supreme Court decisions on public opinion. That both are strongly positive suggests the existence of a responsive Court whose decisions not only reflect changes in public opinion but also serve to reinforce and legitimize opinion change in an iterative process.

The impact of broad trends in the ideology of the public mood on the ideology of the Court's decisions appears to be mediated at least partially through the effects of public opinion on the ideological composition of Congress and the party of president and, via these linkages, through changes in the ideological composition of the Supreme Court. Nevertheless, the evidence suggests that public opinion exercises important influence on the decisions of the Court even in the absence of changes in the composition of the Court or in the partisan and ideological make up of Congress and the presidency. That the effects of public opinion take five years, on average, to register on the Court's decisions probably reflects both the time it takes for a change in public opinion to be reflected in presidential elections and the time required before a newly elected president has a Court vacancy to fill. Even where the Court adjusts its decisions in the absence of membership change, it is likely to take the justices several years to perceive, interpret, and react to fundamental changes in the public mood.

From this vantage, then, members of the Court appear to be politically, as well as ideologically, motivated. Although the Court's decisions are driven

$$\sum_{j \neq i} p_{ij}(u_i - u_j)$$

is called the *prospective rating* of candidate i, with respect to the voter type u and the pivot probability vector p. Under plurality rule, where a voter can set one component of v equal to one, and must set all other components to zero, a voter of type u maximizes his expected gain G(p, v, u) over all permissible ballots v in V by voting for the candidate who has the highest prospective rating. Under approval voting, where a voter can give one vote each to as many candidates as he wants, G(p, v, u) is maximized by giving a vote to every candidate who has a positive prospective rating and by not voting for any candidate who has a negative prospective rating. Under Borda rule, G(p, v, u) is maximized by ranking the candidates on the Borda ballot in order of their prospective ratings.

An *election result* is a probability distribution μ over the set V × T that summarizes the aggregate voting behavior of voters of each type. For any v in V and any u in T, μ(v, u) is the fraction of the electorate consisting of voters of type u who cast the ballot v. Therefore,

$$\mu(v, T) = \sum_{u \in T} \mu(v, u)$$

is the fraction of all voters in the electorate who cast the ballot v. Furthermore, the marginal distribution of μ on the type set T is the population distribution f; that is, for every voter type u,

$$\sum_{v \in V} \mu(v, u) = f(u).$$

In what follows, we shall view an election as the result of a large number of random draws from μ. This will allow us to differentiate between two-candidate races that are "taken seriously" (i.e., the perceived chance of a tie between the candidates is positive) and races that are "close" (i.e., the actual outcome of the election is a near tie for victory between the candidates).

Given an election result μ, the *predicted score* (i.e., expected per-capita vote total) of candidate i is

$$S_i(\mu) = \sum_{v \in V} v_i \cdot \mu(v, T).$$

A *likely winner* is a candidate whose predicted score is maximal. We denote by W(μ) the set of likely winners when the election result is μ; that is, candidate i is in W(μ) if $S_i(\mu) = \max_{j \in K} S_j(\mu)$.

For any pivot probability vector p, we define the *voter response set* R(p) to be the set of all election results in which all of the voters cast ballots that maximize their expected gains, given that their common perception of the pivot probabilities is p. (So far, we have been considering a pivot probability vector p to represent the perceptions of an individual voter concerning the likelihood of various pairs of candidates' being the primary contenders to victory. In what follows, we shall be assuming that all voters hold the same perceptions.) An election result μ is in R(p) if and only if for every ballot v in V and voter type u in T,

$$\text{if } \mu(v, u) > 0 \text{ then } G(p, v, u) = \max_{w \in V} G(p, w, u).$$

If p denotes the vector of pivot probabilities as perceived by the voters at the time of the election and all voters choose their ballots according to the principle of expected utility maximization, then the result of the election should be a distribution of votes by voter type that lies in the voter response set R(p). If an election result μ is in R(p), then we say that the pivot probability vector p *justifies* μ. For future reference, we note here that if p justifies μ and if q is another pivot probability vector satisfying $q_{ij} = \alpha \cdot p_{ij}$ for some $\alpha > 0$ and all candidate pairs ij, then q also justifies μ, because the expected-gain function G is homogeneous in p.

VOTING EQUILIBRIA

The voter response mapping R(·) indicates how potential election results depend on the voters' perception of the pivot probabilities; and the likely-winner mapping W(·) indicates how the set of candidates with serious chances of winning depends on the election result. It remains for us to characterize the pivot probability vectors that may be perceived by the voters in equilibrium, given an electoral situation described by (K, V, T, f).

When the electorate is large, the probability of an election's being nearly tied is typically very small. However, if the pivot probabilities were all perceived to be zero, then an individual voter's action would not matter and any ballot would be optimal for any voter. We can avoid this trivial situation by requiring that for every pair of candidates {i, j}, the ij–pivot probability is a positive number, although it may be very small. (Weakening this assumption does not eliminate any voting equilibria, but we shall see that it can allow new equilibria to arise.)

On the other hand, if the voters can predict the scores ($S_1(\mu)$, $S_2(\mu)$, . . . , $S_K(\mu)$) of the various candidates relatively accurately (e.g., as a result of preelection polls that have very small standard errors), then some pivot probabilities should be perceived to be much smaller than others. It seems reasonable to assume that voters would expect candidates with lower predicted scores to be much less likely to be serious contenders for first place than candidates with higher predicted scores; that is, if the predicted score for some candidate i is strictly less than the score for some other candidate j, then the voters would perceive that candidate i's being tied for first place with any third candidate h is much less likely than candidate j's being tied for first place with candidate h. Given an election result μ and any $0 \leq \epsilon < 1$, we say that a pivot probability vector p satisfies

A MODEL OF MULTICANDIDATE ELECTIONS

We begin by defining an abstract model of an election. There are k candidates, whom we number consecutively to form the set $K = \{1, 2, \ldots, k\}$. Each voter submits a ballot, which is a vector of the form $v = (v_1, \ldots, v_k)$, where v_1 votes are given to candidate 1, v_2 votes to candidate 2, and so on.[1] The winner of the election is the candidate who receives the largest total number of votes on all submitted ballots.

Let V denote the set of all possible ballots that a voter could submit, under the rules of the election. In order to simplify notation, we will only consider voting systems for which V is finite. If the election is run under *plurality rule,* then each voter can cast a single vote for, at most, one candidate. Thus, in a three-candidate election with plurality rule,[2]

$$V = \{(1, 0, 0), (0, 1, 0), (0, 0, 1), (0, 0, 0)\}.$$

Under the rules of *approval voting,* a voter can give one or zero votes independently to each of the candidates (see, e.g., Brams and Fishburn 1978, 1983; Merrill 1988; Weber 1977). In a three-candidate election with approval voting,

$$V = \{(1, 0, 0), (0, 1, 0), (0, 0, 1), (1, 1, 0),$$
$$(1, 0, 1), (0, 1, 1), (1, 1, 1), (0, 0, 0)\}.$$

Under *Borda rule* voting, a voter who does not abstain gives zero votes to one candidate, one vote to another, two votes to a third, and so on, so that with three candidates,

$$V = \{(2, 1, 0), (2, 0, 1), (1, 2, 0), (1, 0, 2),$$
$$(0, 1, 2), (0, 2, 1), (0, 0, 0)\}.$$

To represent formally the diversity of interests present within the electorate, we assume that each voter has preferences that can be summarized by a utility vector of the form $u = (u_1, \ldots, u_k)$, where u_i denotes the payoff (on some Von Neumann–Morgenstern utility scale) that the voter would receive if candidate i were to win the election.[3] We refer to a voter's vector of utility payoffs as his *type.* We further assume that there is some finite set T such that $T \subseteq \mathbb{R}^k$ and every voter's type is in the set T.[4] Let f(u) denote the fraction of all voters in the population whose type is the vector u. Then f(·) is a probability distribution over T that summarizes the preferences of the electorate.[5]

We assume that each voter follows the principle of expected utility maximization to determine which ballot in V he will submit in the election. Under our assumptions, his vote only influences his payoff if it changes the winner of the election. Therefore, to apply the criterion of expected utility maximization, a voter needs to estimate the probability that any given pair of candidates may be in a sufficiently close race for first place that his ballot alone could swing the election from one to the other. In order to simplify the following discussion, we will say that two candidates are "tied" if their vote totals differ by 1/2 vote or less. (This convention saves us from needing to give special consideration to voting systems that permit the casting of fractional votes.)

Let H denote the set of all (unordered) pairs of distinct candidates. We denote a pair {i, j} in H, for short, as ij (so ij = ji); there are $k(k - 1)/2$ such pairs. For each pair of candidates i and j, the *ij–pivot probability* p_{ij} is the probability (perceived by a voter) of the event that candidates i and j will be tied for first place in the election. We assume that a voter perceives the probability that he will change the winner of the election from any candidate i to another candidate j by casting a ballot v with $v_i \geq v_j$, to be linearly proportional to $v_i - v_j$, and that the constant of proportionality (the ij–pivot probability) is the same for the perceived chance of changing the winner from j to i if $v_j \geq v_i$. (This is equivalent to assuming that, conditional on two candidates' being in a close race for first place, the difference in their vote totals is perceived to be approximately uniform near zero on the lattice of possible vote differences, so that the probability of i and j's being tied for first place is essentially the same as the probability of i's being in first place one vote ahead of j, which is also essentially the same as the probability of j's being in first place two votes ahead of i, and so on. For this assumption to be reasonable, it is only necessary that the electorate not be too small.) We also assume that the perceived probability of three candidates' being tied for first place is infinitesimal in comparison to the probability of a two-candidate tie.

A vector that lists the perceived pivot probabilities for all pairs of candidates is denoted $p = (p_{ij})_{ij \in H}$. A voter with ij–pivot probability p_{ij} will perceive the probability that he might change the winner of the election from j to i by submitting the vote vector v to be $p_{ij} \cdot \max(v_i - v_j, 0)$.

Let G(p, v, u) denote the utility gain expected by a voter of type u from submitting the ballot v when p is his vector of perceived pivot probabilities. It follows from our assumptions that

$$G(p, v, u) = \sum_{ij \in H} p_{ij}(v_i - v_j)(u_i - u_j);$$

that is, for each ij pair, if $v_i > v_j$, then by casting the vote vector v (rather than abstaining) the voter has probability $p_{ij}(v_i - v_j)$ of changing the winner of the election from j to i, which gives him a net utility gain of $u_i - u_j$. Multiplying the utility gains by the corresponding probabilities and summing over all pairs of candidates gives the expected gain.

The expected-gain formula can be rewritten as

$$G(p, v, u) = \sum_{i \in K} v_i \sum_{j \neq i} p_{ij}(u_i - u_j).$$

The expression

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A THEORY OF VOTING EQUILIBRIA

ROGER B. MYERSON and ROBERT J. WEBER *Northwestern University*

A voting equilibrium arises when the voters in an electorate, acting in accordance with both their preferences for the candidates and their perceptions of the relative chances of various pairs of candidates being in contention for victory, generate an election result that justifies their perceptions. Voting equilibria always exist, and the set of equilibria can vary substantially with the choice of voting system. We compare equilibria under the plurality rule, approval voting, and the Borda system. We consider a candidate-positioning game and find that the plurality rule imposes little restriction on the position of the winning candidate in three-candidate races, while approval voting leads to a winner positioned at the median of the voter distribution. We contrast campaign activities intended to influence voter preferences with activities meant to influence only perceptions of candidate viability.

Voters in an election typically have not only personal preferences for the various candidates, but also perceptions (influenced by news reports, public opinion polls, and commonly heard talk "on the street") of the candidates' relative chances of winning the election. The behavior of a voter may depend on both. For example, when many candidates are running for the same office under plurality rule, a voter might not vote for the candidate that he most prefers if this candidate is considered unlikely to be in contention for victory.

A voter's honest response to a poll asking his voting intentions might well depend upon perceptions formed from viewing the results of previous polls. Voting intentions may change from day to day as the voters' perceptions evolve during a campaign (even if their preferences remain unchanged), so that a poll, when published, may invalidate itself. A *voting equilibrium* arises when the perceptions arising from the publication of a poll lead the voters to behave in a manner that in turn justifies the predictions of the poll.

Voting equilibria based on polls are somewhat analogous to competitive equilibria based on prices in economic markets. In a pure exchange economy, buyers have preferences for various bundles of commodities. Trading behavior, which generates prices, is driven by the buyers' perceptions of the prices they will confront. At equilibrium, prices summarize demand, and simultaneously generate that same demand. The study of competitive equilibria does not require specification of the precise mechanism by which individual market participants adjust their behavior as the market moves toward equilibrium. Similarly, a detailed model of the process by which a voting equilibrium might be reached is not essential to the definition and computation of voting equilibria. Still, just as one expects markets to clear at equilibrium prices eventually, one might expect that voters will ultimately behave in accordance with a voting equilibrium after observing the series of public reports that accompany an extended campaign. (The campaign is, in part, a political *tâtonnement*, or equilibrium-seeking, process.)

Following the terminology of Simon (1954), we say that an election displays a *bandwagon effect* if voters become more inclined to vote for a given candidate as his standing in preelection polls improves. Conversely, it displays an *underdog effect* if the voters become less inclined to vote for a candidate as his standing in the polls improves. The possibility of bandwagon or underdog effects has been recognized and investigated in the received literature on voting. In particular, Simon showed that there can be many different predictions that are self-fulfilling prophecies, especially when there is a bandwagon effect (see also Bowden 1987; Grunberg and Modigliani 1954; McKelvey and Ordeshook 1985).

In most of the literature on elections involving three or more candidates, however, bandwagon and underdog effects are exogenously specified, rather than endogenously derived from the voters' preferences and the nature of the electoral system. Here, we develop a simple decision-theoretic model to describe how voters' actions might be based on their preferences and on perceptions derived from preelection polls; and we characterize equilibrium outcomes under different electoral systems for multicandidate elections. We use this model to show that different electoral systems may have very different sets of voting equilibria, even when the voters' preferences are held fixed. We also present a simple model of spatial competition among candidates and show that the choice between plurality rule and approval voting can lead to substantial differences in the equilibrium positioning of candidates. Finally, we discuss the difference between campaign activities intended to present the candidates' positions to the electorate (and thus to help voters determine their preferences) and activities that attempt to influence the selection of an equilibrium outcome (by manipulating the voters' perceptions of relative candidate viability).

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William Mishler is Professor of Political Science, University of South Carolina, Columbia, SC 29208.

Reginald S. Sheehan is Assistant Professor of Political Science, Michigan State University, East Lansing, MI 48824.

Court's composition and its decisions should increase with the increasing popularity of Democratic presidents and should decrease with the increasing popularity of Republican presidents.

13. Four measures of the partisan and ideological composition of Congress were considered. The percentages of seats controlled by Democrats in the Senate and the House of Representatives were used as indicators of the partisan orientation of Congress. The percentages of all Senate and House roll-call votes won by the conservative coalition of Republicans and southern Democrats in opposition to a majority of northern Democrats were used as (negative) indicators of the liberal ideological orientation of Congress. Because the Senate and House measures were highly correlated and had virtually identical effects on Court decisions, we report only the House data in this paper.

14. To facilitate comparisons, all measures in Figures 1 and 2 have been standardized with a mean of 100 and a standard deviation of 10.

15. Initially, lags up to 10 years were examined. However significant relationships were not found at lags longer than five years. In the interest of simplicity, only lags up to five years are reported in the tables.

16. The Chow statistic measures whether a significant shift has occurred in the parameters of a model across two time periods. It operates, in effect, by comparing the residual sum of squares for the entire model with the residual sum of squares for the first of the two time periods alone, controlling the different degrees of freedom. More formally,

$$F^{m}_{(n-k)} = \frac{(SS - SS_1)/m}{ss_1/n - k},$$

where SS is the residual sum of squares for the entire period (1956–89), SS_1 is the residual sum of squares for the first part of the period (1956–80), m is the number of observations excluded in calculating SS_1, n is the number of observations included in calculating SS_1, and k is the number of parameters estimated in the model. The Chow statistic is distributed with an F-distribution with m and n - k degrees of freedom.

To confirm that the appropriate break point in the series is 1980 and not some other year, we calculated three additional Chow tests. First, on the assumption that the parameters in the model might have changed before 1980 (e.g., in 1969 with the election of President Nixon and the appointment of Chief Justice Warren Burger), we undertook a Chow test of the stability of the model's parameters across the periods 1956–68 and 1969–89. The test was statistically significant at the .05 level (albeit barely), suggesting some instability, though less than in the comparison of the pre- and post-1980 periods. Second, the Chow test comparing the pre- and post-1969 periods was repeated without the years 1981–89. The result was not statistically significant. This strongly suggests that the Reagan years were very likely the source of the instability in the model's parameters and increases confidence that the model's parameters are reasonably stable through 1980 but differ significantly during the Reagan years.

17. Because of the presence of significant autocorrelation in these series, all of the models were estimated initially using full maximum likelihood techniques (Ostrom 1990, 35). The original estimates indicated that the autoregressive terms were not statistically significant in models C and D. Therefore we reestimated these models using Ordinary Least Squares procedures. The statistics at the bottom of each model summarize the model's performance. The Durbin–Watson statistic is a standard test for first-order autocorrelation in a model's residuals. The Lung Box Q statistic is calculated from the residual autocorrelation function and measures the extent of systematic variation in a model's residuals. If significant, it indicates that there is additional information in the residuals that is not captured up by the model. If the Lung Box Q statistic is not significant, then it is reasonable to conclude that the residuals are "white noise." R^2(tot) measures the total variance reduction achieved by a model including the autoregressive term. R^2(reg) measures that part of the reduction in total variance associated with the structural model (i.e., after correction for the variance associated with the autoregressive term or noise model).

18. In essence, assessments of Granger causality (Freeman 1983; Granger 1969) require that lagged values of X account for significant variance in current values of Y that cannot be explained by past values of Y. In other words, we may infer that public opinion "Granger-causes" Court decisions to the extent that (1) there are significant lagged effects of public opinion on Court decisions, "controlling" via the autoregression term for prior values of the Court's decisions, and (2) these lagged effects produce significant reductions in the total variance explained by the equation.

19. That the interactive intercept terms in these equations are generally positive and large is of no particular substantive significance. In effect, they are statistical artifacts of the other variables in the equations; they indicate the baseline from which the effects of the other variables are measured.

20. Time series methods require that a series be stationary both in its mean and variance. Diagnostics indicated that the series measuring the composition of the Court was nonstationary in its mean. To compensate, this series was first-difference prior to analysis. Because the Court composition variable was differenced, it was necessary to difference the independent variables, as well. In substantive terms, this means that rather than explaining the ideological composition of the Court, our analysis is explaining changes in that composition.

21. According to the criteria for Granger causality the absence of lagged effects in this relationship makes it impossible to assess the direction of the relationship between the party of the president and the composition of the Court or to attribute causality. Nevertheless, although it is conceivable that the ideological composition of the Court can influence subsequent election outcomes and thus the party of the president, such effects, if they exist, are likely to be lagged several years or more. In contrast, it is much more likely that a change in the party of the president will have a more immediate effect on the composition of the Court, as, indeed, was the case in 1969 when President Nixon appointed Warren Burger to fill a vacancy on the Court shortly after his inauguration.

22. Although it would be nice to report that this result was the product of clever theorizing, in fact its discovery was serendipitous. A programming error led to the initial observation that the undifferenced party variable produced a much better fit. Tests of the other variables in the model indicated that the initial specifications (i.e., the differenced versions) were the better-fitting specifications in every case. Of course, having stumbled on the specification of the party variable, our explanation of its meaning is necessarily post hoc.

23. There is some speculation, based on several cases in the most recent (1991–92) session of the Court, that at least some members of the Court may be adjusting their views in a manner consistent with the direction of the public mood. Most frequently mentioned in support of this position are the decisions in *Lee* v. *Weisman* (1992) and *Planned Parenthood of Southeastern Pennsylvania* v. *Casey* (1992), in which conservative Justices Kennedy, Souter, and O'Connor voted to strike down prayer in high school graduation ceremonies and to strike provisions of a state abortion law requiring spousal notice. Although these justices' and the Court's decisions in these cases surprised many observers, it is much too early to conclude from these more-moderate-than-expected results that members of the current Court either individually or collectively are responding to recent trends in public opinion.

References

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is called on to settle other issues, it may become harder or easier for the Court to decide the case in a liberal direction, thus causing the percentage of liberal outcomes to rise or fall. Second, the mix of cases (criminal vs. civil liberty vs. economic regulation, etc.) that are decided by the Court also can vary over time because of changing societal conditions and concerns, as well as the changing agenda of the members of the Court. Together, these raise the possibility that a given percentage of liberal decision outcomes in one year may reflect a significantly more liberal or a significantly less liberal, policy position than would an identical percentage of liberal outcomes in a different year (see, esp., Baum 1988, 1992).

The usual way to handle these problems is to focus on cases in a single-policy domain (thereby limiting agenda effects) and correct for changing case content either by introducing statistical controls for specific case characteristics (Segal 1984, 1985; Atkins and Sloope 1986) or by adjusting for changes in the median votes of continuing justices (Baum 1988). Unfortunately, our interest in looking broadly at changes in the Court's ideology dictated our decision to aggregate across policy areas. However, to test the sensitivity of our measure to agenda effects, we compared outcomes in different policy areas (criminal, civil liberties, etc.) over time. The high correlations (average r ≥ .85) suggest that agenda effects are probably relatively modest. We also introduced controls in some of our early multivariate models for the percentages of different types of cases heard by the Court each year. The controls, generally, were not statistically significant and did not alter the interpretation of results. This reinforces confidence that agenda effects are not significant. We drop these controls in the models reported in our analyses, because they did not add to the models and consumed numerous degrees of freedom.

Because we aggregate across policy types, it is not feasible to control effectively for case content effects by introducing case characteristics. The characteristics that distinguish criminal cases are obviously different from those that distinguish economic regulation cases. To control simultaneously for the myriad relevant case characteristics in many different policy areas would quickly exhaust the limited available degrees of freedom. It would be possible to adopt Baum's (1988) innovative strategy of controlling for case content by adjusting collective outcomes by the median change in the votes of continuing justices from one year to the next. Unfortunately, this strategy assumes that all change in collective outcomes is a function either of membership change on the Court or of changes in case content. The ideologies of individual justices are assumed to be invariant over time. Thus, to adopt this strategy would prevent our consideration of the political adjustment hypothesis that the Court may adapt its decisions to public opinion at the margins even in the absence of membership change. Moreover, in his study of civil liberties cases, Baum reports that the correlation between his content-adjusted measure and an unadjusted measure of collective outcomes is very high (r = .88; 1988, 910). This suggests that the problem of content effects may not be as severe in practice as it is in theory.

Given the absence of any significant evidence of agenda effects and the difficulty of controlling for content effects without introducing difficult limiting assumptions, we use the collective outcome of Supreme Court decisions as a surrogate measure of the policy ideology of the Court. The measure is imperfect, to be sure; but we contend that it does provide a reasonable basis for examining broad trends in the ideology of the Court's decisions over time. In support of this contention, we would point to the obvious face validity of the measure as illustrated in Figures 1 and 2 and as discussed in the subsequent text.

8. As a check on the consequences of this decision, we constructed a second index using all of the Court's decisions, including those decided *per curiam* or with memorandum opinions. We also constructed a policy-specific measure of the liberalism of the Court in criminal cases. Although there may be debate in other policy domains as to whether a particular decision is liberal or conservative, a decision in favor of the defendant in a criminal case is almost always considered to be liberal. That all three measures are highly correlated (r ≥ .90 for each pair) attests to the construct validity of the original measure and increases confidence in its interpretation as a composite indicator of the liberalism of the Court's decisions.

9. In an earlier version of this study, two additional measures of public opinion were considered: (1) political party identification (the percentage of the public identifying themselves at Democrats in yearly Gallup polls) and (2) self-identified ideology (the percentage of individuals identifying themselves as liberals in biennial National Election Studies). In brief, our analyses indicated that the public mood was much more strongly related to variations in the Court's decisions. Indeed, there were no significant correlations between either party identification or self-identified ideology and the liberalism of Supreme Court decisions at lags of 0–10 years. As we shall show, the links between the public mood and the Court's decisions are much stronger. Only the results from the analysis of the public mood are presented here. However, the expanded version is available from the authors on request.

10. In the process of constructing the summary index, Stimson developed a number of policy-specific components of mood, including an index of attitudes on crime, civil liberties, and gun control and another on abortion. Both have obvious implications for Supreme Court decisions. Unfortunately, because of the paucity of relevant opinion data Stimson's crime index is unavailable before 1963; and the abortion index is unavailable before 1973. Obviously, inclusion of these data would seriously reduce the already small number of cases available for analysis. Moreover Stimson demonstrates that trends in public opinion in most issue areas closely follow the overall pattern observed for the public mood (1992, 69–79). For example, a factor analysis of nine issue specific indices (excluding abortion, on which there were too few cases for analysis) produces two factors, one accounting for nearly 65% of the total variance in series, the other, less than 20%. All nine of the issue areas had loadings of .42 or higher on the first factor; six of the nine had loadings greater than .85. Only the crime, civil liberties, and gun control series loader higher on the second dimension than the first.

Given the loss of cases that would result from the use of issue-specific opinion measures, combined with the evidence that most, if not all, of these issue specific measures closely parallel the pattern observed for the more general measure of the public mood, we restrict our focus here to the general measure of the public mood. However, research in progress undertakes a similar examination of the links between public opinion and Supreme Court decisions in the areas of criminal procedures and civil rights, trying to match the Court's decisions in these cases with Stimson's measures of racial attitudes and public opinion on crime, civil liberties, and gun control for the much shorter time periods for which these data are available (Link 1992). The results of these preliminary analyses generally reinforce those reported here, albeit with slight differences in the length of the observed lags, especially for president and Congress. Significantly, nothing in these analyses undermines the current results or the interpretation given them.

11. Justices were assigned an ideological score on a scale of 1 (*most liberal*) to −1 (*most conservative*). There were a few justices early in the period covered by this study not included in Segal and Cover's (1989) analysis. We estimated the ideology scores for each of them based on the scale analyses reported by Rohde and Spaeth (1976). For elaboration of this method see Sheehan, Mishler, and Songer 1992.

12. Two indices of presidential partisanship were created. First, we constructed a simple dichotomous measure of party control of the White House, coded 1 during the years of a Democratic administration and 0 during periods of Republican administration. A second measure considers the popularity of the president. This was constructed by subtracting 50% from the average job approval rating of the president in any year and multiplying the result by 1 for Republican presidents. The assumption is that the liberalism both of the

several years behind the increasing liberalism of the public mood. By deemphasizing or fuzzing domestic policy issues and concentrating, instead, on labeling Dukakis as a liberal, Bush took advantage of this lag and was able to win the election despite the likelihood that many of his policy positions were inconsistent with an increasingly liberal public mood. Thus, following his election, when Bush appointed justices who shared his conservative ideology, the effect was to push the Court still further out of alignment with the changing public mood.

Mr. Dooley may have been correct that the Supreme Court follows the election returns. However, it does not necessarily follow, as Dahl and Funston apparently assume, either that election returns provide an accurate reflection of the ideological mood of the country or that the electoral process ensures that the president's appointments will conform to the prevailing mood.

In arguing that the Court is "inevitably a part of the dominant political alliance" and cannot long exist as a countermajoritarian institution, Dahl assumed the existence both of a dominant political alliance and what he described as a "normally divided" Court. He assumed that the president and Congress would represent the same electoral coalition and that given a reasonable ideological balance on the Court, the ability of a president to appoint an average of two justices to the Court would usually be sufficient to tip the ideological balance of the Court in the direction of the dominant political alliance. Contrary to these assumptions, however, the United States has experienced divided government for all but four of the past 24 years. Moreover, because Republican presidents have filled all of the vacancies on the Court during this period and have appointed all but one of the current justices, the Court can no longer be considered reasonably balanced.

Dahl gave little consideration to the assumption of a balanced Court in his analysis; he mentioned it only in passing. Further reflection suggests, however, that the assumption is critical. In the absence of a reasonably balanced Court, serious questions exist about the effectiveness of either of the pathways by which public opinion has historically influenced the Court. Even if the Democrats were to regain control of the White House, there is little likelihood (given the current ideological imbalance) that sufficient vacancies would occur on this youthful Court to enable the balance to be restored, much less tipped in a liberal direction, in the foreseeable future.

Moreover, in the absence of a reasonably divided Court, it is doubtful whether pressures on the Court to respond directly to public opinion are sufficiently strong to have any meaningful impact on the Court's decisions. A change in public opinion sufficient to change the vote of a single justice at the margin can be of extreme significance if the Court is balanced roughly five to four. The significance of such a change for a Court divided six to three or seven to two is considerably more questionable.

Given the obvious limits of this analysis, we cannot do more than speculate about these fundamental issues. Thus, although analysis of the Warren and Burger years generally paints a picture of the Court as an institution that has responded in deliberate fashion to majority opinion, informed speculation about current and foreseeable future trends raises disturbing though still unanswered questions about the role of the Court as a countermajoritarian institution.

Notes

We would like to thank Jonathan Casper, William Jacoby, Patricia Machado, Nancy Maveety, Donald Songer, and C. Neal Tate for their constructive comments on earlier drafts.

1. Casper also extended Dahl's analysis across time. Although he added only 14 years to the 167 considered by Dahl (ending in 1974, rather than 1957), those additional years include most of the Warren Court, whose behavior, Casper argues, was distinctive.

2. Indeed, although there are questions about whether the relationship is positive or negative, there is relatively greater evidence in the literature of judicial decisions' influencing public opinion and political support for the Court than of reciprocal effects. See, e.g., Adamany and Grossman 1983; Caldeira 1986; Dolbeare 1967; Kessel 1966; Lehne and Reynolds 1978; Marshall 1989; Tanenhaus and Murphy 1981).

3. Importantly, the political adjustment hypothesis does not require the public to be attentive to, or informed about, specific decisions of the Court. Indeed, much research shows that the public is neither. Neither is it necessary for the justices to believe in the myth of an attentive and knowledgeable public. What is important is a perception by one or more justices (the number depending on the ideological division on the Court) that the public has a general awareness of the broad ideological tenor of the Court in important issue areas and that the legitimacy of the Court and its moral force may be jeopardized by a pattern of decisions that contradict deeply held and abiding public beliefs or concerns.

4. This paragraph borrows heavily from O'Connor and Sabato 1993, 334–36). That the political adjustment hypothesis is standard fare in American politics textbooks is indicative of the widespread acceptance it enjoys notwithstanding the dearth of systematic evidence.

5. As will be noted, public opinion data for most specific policy areas are also in extremely short supply. Reliable data on most are unavailable in consistent time series before the middle 1960s. This, of course, compounds the problem of attempting to establish relationships between public opinion and the Court's decisions in specific policy domains over time. Thus, we have proceeded in stages, examining first the impact of general trends in public opinion on the overall ideological tone on the Court's decisions and delaying for a later analysis the more complicated task of looking at specific policy areas. Nevertheless, for a very preliminary effort at the latter, see Link 1992.

6. The criteria used in determining whether a particular decision is liberal or conservative are described in some detail by Spaeth (1991). In general, liberal decisions include those favoring unions, consumers, environmental protection, economic "underdogs," and individuals bringing civil liberties cases. Decisions made against these interests are generally coded as conservative. In cases involving economic regulation, liberal decisions are those decided in favor of the government. If the liberal or conservative direction of a decision could not be determined, the case was excluded from the analysis.

7. Measuring the liberalism of Court decisions over time is complicated by at least two additional considerations. First, the policy content of similar cases before the Court may change over time. For example, as the Court "settles" one set of issues with respect to the rights of criminal defendants and

substantially by the ideologies of its members, fluctuations in the ideological tenor of public opinion and in the ideological and partisan orientations of the president and Congress are also important. Clearly, the ideology of the Court occupies a critical position. Just as clearly, public and elite opinion are linked directly to the Court and influence its decisions even in the absence of changes in Court membership. At least, such are the patterns for the period 1956–81.

Normatively, the implications of this analysis are much less clear. Given its preliminary nature, a lengthy discussion of meanings is probably not warranted. Nevertheless, since we began with a brief discussion of the "countermajoritarian difficulty" posed by an unelected Court wielding substantial political power in a democratic society, it seems appropriate to speculate briefly on this issue in conclusion.

On the most superficial level, the results appear to reassure those committed to democratic principles and concerned about the countermajoritarian potential of the Court. Our analyses indicate that for most of the period since 1956, the Court has been highly responsive to majority opinion. Its decisions not only have conformed closely to the aggregate policy opinions of the American public but have thereby reinforced and helped legitimate emergent majoritarian concerns.

Reinforcing confidence in the majoritarian tendencies of the Court are the indications that the Court not only has been held responsive to public opinion through the appointment process but has also responded directly to trends in public opinion even in the absence of changes in the composition of the Court. The existence of alternative avenues by which public opinion can influence the Court provides additional protection against countermajoritarian tendencies—or at least, it can be so argued from the evidence up to 1981.

At the same time, those who celebrate the republican principles on which the United States was founded can take some comfort in the evidence that the linkage between public opinion and the decisions of the Court is imperfect and occurs after a significant delay. The five-year lag between changes in majority opinion and the reflection of those changes in the Court's decisions suggests that the Court also serves as a temporary buffer against public opinion, shielding the policy process from public caprice and the passions of the moment. Moreover, it is important to reiterate that our analyses focus only on fundamental changes in the underlying ideological tenor of public opinion. Nothing in this analysis suggests that decisions of the Court are responsive to ordinary fluctuations in opinions about specific policies. We have not examined such linkages and are skeptical of their existence.

A problem with this optimistic appraisal, however, is that it is based on what is effectively a historical case study of only about 30 years of the nation's history. Indeed, our analyses indicate that the relationship of the Court to public opinion has been very different in the years since 1981. The decisions of the Court during the Reagan years were significantly countermajoritarian in direction and appear to have been driven almost entirely by the changing ideological orientation of the Court. The Court grew increasingly conservative across this period despite a liberal resurgence in the public mood. Although the conservative appointments of Presidents Nixon and Ford probably reflected the prevailing public sentiment at the time, the appointments of President Reagan increasingly did not. Moreover, it is highly likely that the conservative ideology of the Court and its decisions have either remained unchanged or grown increasingly conservative during the Bush administration, given the appointments of Justices Souter and Thomas. Thus, there is every reason to believe that the gap between public opinion and the decisions of the Court has continued to widen.[23]

We do not mean to suggest by this that Presidents Reagan or Bush deliberately selected justices whose ideologies were antagonistic to the public mood or even that they employed different criteria for judicial appointments from other recent presidents. It is sufficient to argue that like their predecessors, they appointed justices whose ideologies they found compatible but that their personal ideology or the ideologies of their primary electoral constituencies were inconsistent with the public mood. Nor is it difficult to conceive of situations in which this might occur.

To the extent that presidential elections are decided principally on bases other than the public's domestic policy concerns and the candidates' policy positions (e.g., on the basis of candidate or party images, party identification, or even foreign policy considerations), then it is possible—perhaps even likely—that a president will be elected whose ideology is unrelated, if not antagonistic, to the public mood. Since more than three decades of research on voting choice and election outcomes in the United States indicate that candidate and party images and party identification often play larger roles in determining outcomes than the ideologies of the competing candidates, there are good reasons to believe that elections can produce results that are inconsistent with the ideology of the public mood.

Indeed, Stimson argues that something like this has occurred in the United States in recent years (1992, 79–84). Although the election of Ronald Reagan in 1980 was consistent with the ideology of the public mood, which reached its liberal nadir in 1979 (see Figure 1), the public mood changed direction almost simultaneously with Reagan's election and has become increasingly liberal ever since (at least through 1989, when Stimson's data ends). In Stimson's telling of this tale, President Bush may have been elected in 1988 based on his clever exploitation of a lag that Stimson shows to have existed between the public's self-identified ideology and the liberalism of the public mood in the middle 1980s. Simply stated, increases in self-identified liberalism lagged

TABLE 3

The Impact of Changes in Public Opinion on Year to Year Changes in the Ideological Composition of the Supreme Court, 1958–1989

	MODELS	
	A	B
Intercept	−.31* (.15)	−.66** (.17)
Public Mood$_{t-6}$[a]	.09** (.02)	.05* (.02)
Democratic President[b]		.81** (.23)
Democratic President's Popularity$_{t-1}$[c]	—	.00 (.04)
Conservative Coalition$_{t-4}$[d]	—	−1.78** (.54)
Reagan Intercept[e]	.29 (.28)	.63** (.22)
Reagan × Mood$_{t-6}$	−.08* (.03)	−.01 (.03)
R^2	.39	.76
Durbin-Watson	2.21 (ns)	2.41 (ns)
Lung Box Q (df = 12)	8.11 (ns)	8.25 (ns)

Note: Standard errors are in parentheses. N = 34.
All estimates are ordinary least squares.
[a] Yearly change in the index of the liberalism of public opinion on domestic policy concerns.
[b] Dummy variable scored 1 if a Democrat is president, 0 otherwise.
[c] Yearly change in the partisan-controlled index of presidential approval (see n. 18).
[d] Yearly change in the percentage of roll call votes in which a majority of Republicans and southern Democrats opposed and defeated a majority of northern Democrats in House of Representatives.
[e] Dummy variable coded 1 for years 1981 through 1989 and 0 otherwise.
*P ≤ .05 (one-tailed test).
**P ≤ .01 (one-tailed test).

ideological composition of Congress significantly influence changes in the ideological composition of the Court.[20] Predictably, the party of the president appears to have a stronger impact on the composition of the Court.[21] Unlike the other variables in the model, the party variable was not differenced prior to analysis.[22] This means that the impact of party in the model increases steadily across the length of a president's term. The longer a party controls the White House, the more vacancies on the Court the president is likely to be able to make and the greater the impact the president can have on the Court's ideological orientation.

Unlike the previous model, the impact of public opinion on the ideological composition of the Court does not appear to vary appreciably across the two periods. Public opinion has a small but significant and positive impact on the composition of the Court across most of the time period. The negative coefficients for public mood during the Reagan years suggest a different relationship during this period, but the coefficients are too small to be of consequence.

Furthermore, the coefficients for public opinion in both periods are substantially attenuated by the introduction of controls for the party of the president and the ideological composition of Congress. Although the positive impact of public opinion on the Court's composition prior to 1981 remains significant after introducing the controls, its coefficient is reduced by almost half. This suggests that the effects of public opinion on the composition of the Court are limited and mediated substantially through the impact of public opinion on the party of the president and the composition of Congress.

DISCUSSION

The relationship of public opinion to the Supreme Court appears to be both subtle and complex. We cannot do justice to these subtleties here, nor do we claim to have done so. Instead, we have attempted in this analysis to provide a broad overview of the relationship and to begin the process of identifying directionality and causality.

Empirically, the results of our analysis are easily summarized. For most of the period since 1956, a reciprocal relationship appears to have existed between the ideology of the public mood in the United States and the broad ideological tenor of Supreme Court decisions. The impact of the public mood on the Court's decisions occurs at a moderate lag; a somewhat shorter lag appears to exist in the reciprocal effects of Supreme Court decisions on public opinion. That both are strongly positive suggests the existence of a responsive Court whose decisions not only reflect changes in public opinion but also serve to reinforce and legitimize opinion change in an iterative process.

The impact of broad trends in the ideology of the public mood on the ideology of the Court's decisions appears to be mediated at least partially through the effects of public opinion on the ideological composition of Congress and the party of president and, via these linkages, through changes in the ideological composition of the Supreme Court. Nevertheless, the evidence suggests that public opinion exercises important influence on the decisions of the Court even in the absence of changes in the composition of the Court or in the partisan and ideological make up of Congress and the presidency. That the effects of public opinion take five years, on average, to register on the Court's decisions probably reflects both the time it takes for a change in public opinion to be reflected in presidential elections and the time required before a newly elected president has a Court vacancy to fill. Even where the Court adjusts its decisions in the absence of membership change, it is likely to take the justices several years to perceive, interpret, and react to fundamental changes in the public mood.

From this vantage, then, members of the Court appear to be politically, as well as ideologically, motivated. Although the Court's decisions are driven for differences in the impact of public opinion in the Reagan years, all of the models include an interactive term and associated slope coefficient for public opinion during the period 1981–89.

Model A estimates the impact of the public mood on the liberalism of Supreme Court decisions controlling only for the Reagan years and first-order autoregression.[17] It confirms the results of the cross correlations and indicates that the public mood "Granger-causes" Supreme Court decisions at a moderate lag.[18] The maximum likelihood coefficient is statistically significant at a lag of five years, and the model reduces the variance in the liberalism of the Court's decisions by 35% after adjusting for autocorrelation.

Also notable in model A is the coefficient for the interactive term measuring public opinion during the Reagan years. To determine the effect of the public mood during this period the coefficient for the interactive Reagan-Mood variable must be added to the coefficient for public mood more generally. Thus, in equation A the effect of the public mood during the Reagan years is:

$$Y = (-24.3 + 140.9) + (.77 - 1.59)X.$$

The impact of mood during the Reagan years is negative and about as large as the positive effect during the rest of the 1956–1989 period. That the coefficient for the interactive term is statistically significant indicates that the impact of mood differs significantly during the Reagan years compared to the rest of the period ($b = -.82$ vs .77).[19]

The discrepancy in the impact of public mood on the decisions of the Court before and after 1981 is equally strong in the other models, which control separately for the ideological composition of the Court (model B), the party and ideology of president and Congress (model C), and all of the variables simultaneously (model D). In each of these models, public opinion has strong, positive effects on the Court's decisions prior to 1981 but about equally strong and *negative* effects during the Reagan years.

Regarding the avenues by which public opinion registers on the Court, the evidence in models B and D clearly indicates that the impact of public mood on the Court's decisions survives the introduction of controls for the ideological composition of the Court and for the partisan and ideological orientation of Congress and president. This is the case both for the positive effects of public opinion prior to 1981 and the negative effects of public opinion thereafter.

That these effects are somewhat attenuated by the controls implies that the impact of public opinion on the Court's decisions is mediated partly through its effects on the changing membership of the Court. However, consistent with the political adjustment hypothesis, the persistence of strong public mood effects controlling for the membership of the Court indicates that the impact of public opinion on the Court's decisions is partially independent of membership change, as well.

A final observation in model D is the persistence of significant effects for the party of the president and the conservative coalition in Congress despite controls for the ideological composition of the Court. Here, again, the effects of president and Congress are reduced when the composition of the Court is controlled (compare models C and D), confirming, as expected, that the influence of president and Congress on the Court is mediated partly though the appointment and confirmation process. Again, however, the persistence of significant effects despite this control suggests that the Court responds to presidential and congressional sentiment in the absence of membership change and independently of public opinion, as well.

That the lags in these relationships are shorter for the party of the president and longer for the conservative coalition in Congress supports the expectation that the impact of the president on the Court is more immediate than that of Congress. Nevertheless, the consistency with which the Court's decisions appear to follow the partisan and ideological orientation of the president and Congress during the years prior to 1981 lends credence to Dahl's contention that the Court has traditionally acted as part of the dominant political coalition in America—at least prior to the Reagan years.

What emerges from these data, then, is an image of the Court prior to 1981 as an ideologically motivated but politically sensitive institution. The Court's decisions were influenced substantially by the ideologies of the sitting justices but responded independently to both public and elite opinion, as well. In the years since 1981, however, the Court appears as an even more ideologically driven but politically isolated institution. Its decisions have been independent of, if not antagonistic to, recent trends in the public mood and have been out of alignment with the dominant coalition in Congress. That the Court's decisions since 1981 have remained consistent with the partisan orientation of the president reflects, in all likelihood, the existence of divided government and the absence in the 1980s of any semblance of a dominant political alliance.

THE CHANGING COMPOSITION OF THE COURT

Notwithstanding the significant, independent effects of public and elite opinion on the Court's decisions prior to 1981, the changing ideological composition of the Court's membership clearly has been a principal dynamic underlying the Court's decisions both before and (especially) after 1981. To understand better the effects of public and elite opinion on the composition of the Court, a second set of models was constructed (Table 3).

Consistent with the Dahl–Funston hypothesis, the results indicate that changes in the party (though not the popularity) of the president and changes in the

TABLE 2

The Impact of Public Opinion on Supreme Court Decisions, 1956–1989

	MODELS			
	A[a]	B[a]	C[b]	D[b]
Intercept	−24.3 (30.7)	−1.98 (28.2)	−46.6* (23.9)	−11.95 (25.42)
Public Mood$_{t-5}$[c]	.77** (.30)	.49* (.27)	1.08** (.22)	.66** (.26)
Court Membership$_{t-1}$[d]	—	2.78** (.79)	—	1.76** (.70)
Democratic President$_{t-1}$[e]	—	—	5.41* (3.03)	5.52* (2.89)
Democratic President's Popularity$_{t-3}$[f]	—	—	−.31** (.12)	−.15 (.12)
Conservative Coalition$_{t-4}$[g]	—	—	−.86** (.34)	−.53 (.27)
Reagan Intercept[h]	140.9* (73.7)	90.3 (59.0)	161.3** (50.2)	114.2* (48.8)
Reagan × Mood$_{t-5}$	−1.59* (.81)	−.94 (.56)	−1.78** (.55)	−1.22* (.54)
AR(1)	−.52 (.18)	−.15 (.09)	—	—
R^2(*reg*)[i]	.35	.71	.80	.85
R^2(*tot*)[j]	.68	.78		
Durbin-Watson	1.86 (ns)	1.92 (ns)	2.03 (ns)	2.19 (ns)
Lung Box Q (df = 12)	10.6 (ns)	10.9 (ns)	11.8 (ns)	11.6 (ns)

Note: Standard errors are in parentheses. N = 34.
[a] Maximum Likelihood Estimates.
[b] Ordinary Least Squares Estimates.
[c] Index of the liberalism of public opinion on domestic policy concerns.
[d] Sum of liberalism scores of Supreme Court justices at time of initial appointments.
[e] Dummy variable scored 1 if a Democrat is president, 0 otherwise.
[f] Partisan-controlled index of presidential approval (see n. 18).
[g] Percentage of roll call votes in which a majority of Republicans and southern Democrats opposed and defeated a majority of northern Democrats in House of Representatives.
[h] Dummy variable coded 1 for years 1981 through 1989 and 0 otherwise.
[i] Variance explained by model excluding autoregressive parameter.
[j] Variance explained by model including autoregressive parameter.
*P ≤ .05 (one-tailed test).
**P ≤ .01 (one-tailed test).

process of elaborating relationships. We establish the existence of a relationship between public opinion and Court decisions (model A), then examine whether this relationship survives the introduction of controls for (1) the ideological composition of the Court (model B), (2) the ideological orientations of president and Congress (model C), and (3) all of the variables in combination (model D). Evidence that the relationship between public opinion and the Supreme Court vanishes when controls are introduced would validate the Dahl–Funston hypothesis that public opinion influences decisions of the Court only indirectly through the mediating influence of elections and the orientations of political elites on the ideological composition of the Court. Conversely, evidence that the effects of public opinion survive the introduction of these controls would support the political adjustment hypothesis that public opinion has significant direct effects on the Court's decisions independent of the composition of the Court or the political orientation of president and Congress.

We noted previously that the ideologies of the public mood and of the decisions of the Court have diverged rather sharply since President Reagan's election in 1980 (recall Figure 1). Public opinion grew increasingly liberal across this decade, whereas the Court's decisions remained relatively stable or became slightly more conservative. This departure from the overall correspondence of these series raises the possibility of parameter instability over time. A Chow test (not shown) comparing the residual variance of a preliminary version of model D (without the interactive terms for the Reagan years) for the time periods 1956–81 and 1981–89, was statistically significant, confirming the existence of significant differences in the model before and after 1981.[16] Thus, to account

TABLE 1

Cross Correlations between the Liberalism of Supreme Court Decisions and the Ideological or Partisan Composition of the Public Mood, Presidency, Congress, and Supreme Court

COURT DECISIONS[a]	PUBLIC MOOD[b]	DEMOCRATIC PRESIDENT'S POPULARITY[c]	DEMOCRATIC PRESIDENT[d]	% SEATS DEMOCRATIC[e]	CONSERVATIVE COALITION[f]	IDEOLOGICAL COMPOSITION OF COURT[g]
t − 5	−.07	−.17	.12	−.13	.19	.17
t − 4	.18	−.13	.23	.01	−.20	.30*
t − 3	−.12	.13	−.17	.04	−.08	.07
t − 2	−.14	.18	.10	.25	−.09	.31*
t − 1	.40*	.00	.00	−.20	.15	.37*
t	−.01	.02	.04	−.10	−.20	.28
t + 1	.12	.08	.33*	−.12	.19	.44*
t + 2	.08	.24	.24	.07	−.11	.22
t + 3	.26	.34*	.17	.25	.10	.22
t + 4	.19	.02	−.22	.23	−.30*	.20
t + 5	.36*	.11	.19	−.02	−.14	.28

Note: All variables are filtered by a first-order autoregressive model, $(1 - .73B)y_t = a_t$, prior to analysis.
[a] Yearly percentage of Supreme Court cases decided in a liberal direction.
[b] Index of the liberalism of public opinion on domestic policy concerns.
[c] Partisan-controlled index of presidential approval (see n. 18).
[d] Dummy variable scored 1 if a Democrat is president, 0 otherwise.
[e] Percentage of Democratic seats in the House of Representaitves.
[f] Percentage of roll call votes in which a majority of Republicans and southern Democrats opposed and defeated a majority of northern Democrats in House of Representatives.
[g] Sum of liberalism scores of Supreme Court justices at time of initial appointments.
*$p \leq .05$.

sions are mediated through elite opinion or the changing composition of the Court, Table 1 also reports the cross correlations between the Court's decisions and several measures of the ideological and partisan composition of Congress, the presidency, and the Supreme Court. Both the party of the president and a partisan-controlled measure of the president's popularity are moderately correlated with the Court's decisions—party at a lag of one year and popularity at a lag of three years. In contrast, the percentage of seats in the House of Representatives occupied by Democrats is unrelated to the Court's decisions at any lag up to 10 years; but the strength of the conservative coalition in the House is significantly and negatively correlated with the Court's decisions at a lag of four years. In none of these cases is there any evidence of reciprocal effects.

Finally, Table 1 also reports cross correlations between the liberalism of the Supreme Court's decisions and the ideological makeup of the Court's membership. As expected, the correlations are the strongest, and the lags the shortest, in the data. There also is clear evidence of reciprocal effects. The significant, positive correlation at lag t + 1 suggests that changes made in the composition of the Court in one year are reflected in the decisions of the Court beginning the next session. Both the relative strength of the correlation and its short lag are consistent with the Dahl–Funston hypothesis that the composition of the Court is the proximate cause of decision change and an important intervening link to other putative causes.

Equally interesting, however, is the evidence of reciprocal relationships. Significant correlations at lags t − 1, t − 2, and t − 4 suggest that changes in the membership of the Court are as likely to *follow* changes in the Court's decisions as they are to *precede* them. This is not to suggest that changes in the Court's decisions "cause" the membership of the Court to change. Our suspicion is, rather, that the correlations are spurious consequences of simultaneous "political adjustment processes" operating on the president and Congress, as well as the Court.

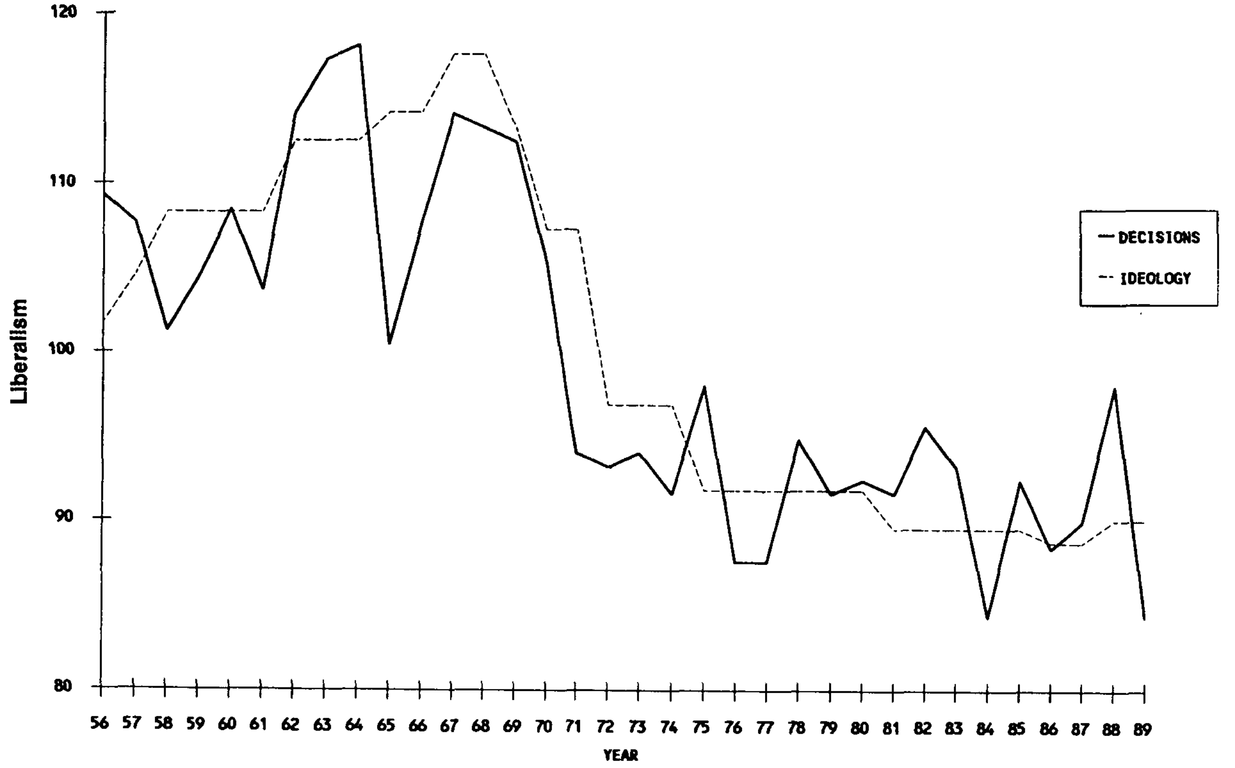
The political adjustment hypothesis holds that members of the Court, perceiving a fundamental shift in public opinion, adjust their decisions at the margins to accommodate that shift. Since the president and Congress are presumably even more sensitive to public opinion, the likelihood is that the same forces that prompt the Court to adjust its decisions will influence the president's judicial appointments and thus the future composition of the Court. In this sense, the Court's decisions not only are responsive to public opinion in the absence of membership change but appear even to anticipate changes in the Court's composition.

MULTIVARIATE MODELS

The lags identified in the cross-correlations were used to specify a series of multivariate models estimating the impact of public opinion on Supreme Court decisions (Table 2). The logic behind models A–D is roughly that of Simon–Blalock causal modeling (Blalock 1961) or what Morris Rosenberg (1968) calls the

FIGURE 2

Standardized Measures of the Ideological Composition of Supreme Court Membership and the Liberalism of Supreme Court Decisions



become convinced that a change is of sufficient salience, magnitude, and endurance as to threaten the Court's authority.

Lacking more specific theoretical guidance, we proceeded empirically to identify the lengths of possible lags. Using Box–Jenkins time-series procedures (Box and Jenkins 1976; Box and Tiao 1975), we identified the autocorrelation in the public opinion series as a simple, first-order autoregessive, or AR(1) process. An AR(1) model then was used to filter the measures of public opinion and the Court's decisions, and the filtered measures were cross-correlated at lags of 0–10 years. Although principally interested in the impact of public opinion on Supreme Court decisions, we examined lags in both directions because of the possibility that the relationship would be reciprocal.

The cross correlations are reported in Table 1. The coefficients in the lower half of the series indicate the correlations between public opinion at time t and the Court's decisions in later years t + 1 to t + 5. Logically, these indicate the potential impact of public opinion on subsequent Court decisions. The correlations in the top half of the series measure relationships between opinion at t and decisions in earlier years t − 1 to t − 5. These reflect possible reciprocal effects of the Court's decisions on public opinion.[15]

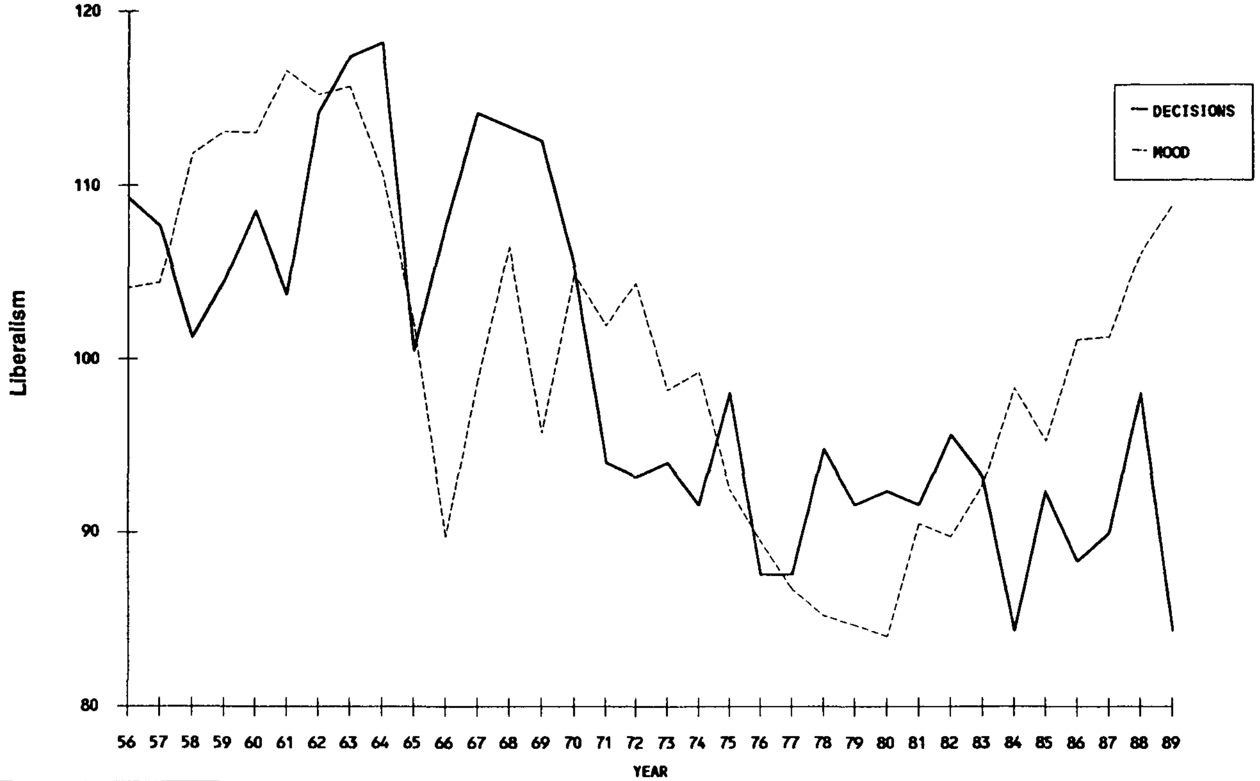
As expected, removing the autocorrelation in these series substantially reduces the zero-order correlation (the middle coefficient); the resulting coefficient is small and statistically insignificant. Nevertheless, the public mood is significantly and positively correlated with trends in the Court's decisions at a lag of five years; and the relationship approaches significance at lag t + 3, as well.

The strong positive relationship between the Court's decisions and the public mood at lag t − 1 also supports the suggestion that the relationship is reciprocal. Although a substantial literature documents the impact of specific Court decisions on public opinion (frequently in a negative direction), the very preliminary evidence here suggests that the broad pattern of Court decisions has a generally positive impact on the public mood. More generally still, this indication of reciprocal links between the public mood and the Court's decisions hints at the existence of a relationship in which (1) the ideological tenor of the Court's decisions are responsive in the long run to the shifts in the ideology of the public mood and (2) the Court's decisions subsequently reinforce and legitimize those shifts in mood. This is highly speculative, of course. Here, we shall concentrate on the possible impact of the public mood on the decisions of the Court and leave the reciprocal relationship to a later study.

As a preliminary step in examining how far linkages between public opinion and the Court's deci-

FIGURE 1

Standardized Measures of the Liberalism of the Public Mood and the Liberalism of Supreme Court Decisions



though the conservatism of the Court overtakes public opinion during the Nixon years, public opinion maintains its gradually conservative trend throughout the 1970s, anticipating by several years the increased conservatism of the Court during the Reagan years. The principal exception to this pattern occurs in the period since 1980, in which the resurgence of liberalism in the public mood is unmatched by any similar trend in the Court's decisions.

Consistent with the emphasis accorded membership change in explaining changes in Supreme Court decisions, Figure 2 demonstrates even greater congruence between the ideological composition of the Court and trends in the liberalism of the Court's decisions ($r = .77$). The strength of this relationship is consistent with the Dahl–Funston hypothesis of the centrality of membership change in explaining fluctuations in the Court's decisions. Nevertheless, although closely aligned, there are indications of relatively short lags in this relationship, as well. Particularly notable is the evidence that the decline in the liberalism of the Court's decisions in the late 1960s began somewhat *before* the first of President Nixon's conservative appointments to the Court. This evidence, however preliminary and tentative, accords with the political adjustment hypothesis that the Court can and does respond to political trends even in the absence of a change in its membership.

LINKAGES AND LAGS

Simple inspection of trend data is useful for obtaining a broad overview of the relationships between public opinion and Supreme Court decisions; but greater rigor is required to estimate the nature and extent of this relationship, determine its timing or lag structure, establish causality, and assess the viability of alternative pathways of influence. Moreover, care must be taken to control for the high levels of autocorrelation found in several of the variables.

Existing theory provides little guide to specifying the lags that might exist in the relationship between the ideological tenor of the public mood and the Court's decisions. Under either of the hypothesized pathways of influence, a significant lag is expected. According to the Dahl–Funston hypothesis, it takes time for a shift in public opinion to be reflected in presidential politics; and additional time is needed for a newly elected president to have opportunities to fill vacancies on the Court. Even where the Court responds directly to perceived changes in opinion as proposed by the political adjustment hypothesis, it still takes time for justices to perceive a change in the public mood. More importantly, it takes time for societal trends to be absorbed and reflected in the attitudes and beliefs of justices or for justices to certainly suggest the plausibility of the political adjustment hypothesis.

METHODS AND MEASURES

To examine the relationship between public opinion and Supreme Court decisions, we use aggregate yearly data for the period 1956–89. Data on Supreme Court decisions are available from the U.S. Supreme Court data base (Spaeth 1991), which includes all Supreme Court decisions during the period of study.

Given both the relatively small number of cases for many specific policy areas in a number of the years under study and our concern with measuring broad ideological trends, we decided not to attempt the development of policy-specific measures of the Court's decisions.[5] Rather, we developed a composite measure of the overall ideological tenor of the Court's decisions each year. The Supreme Court data base codes each of the nearly seven thousand cases decided during this period according to whether its outcome was liberal or conservative in direction.[6] The liberalism of Court decisions in any year is then calculated simply as the percentage of all cases in a year decided in a liberal direction.[7]

In developing an index of the liberalism of Court decisions, we considered only the approximately four thousand cases that were decided by full opinion and oral argument. *Per curiam* cases and cases decided by memorandum were excluded because of concerns that many such cases are highly routine. Such cases are unlikely either to influence, or to be influenced by, public opinion.[8]

To measure the ideological main currents of public opinion across this 33-year period, we relied on Stimson's (1992) innovative index of the liberalism of what he calls the "public mood" in America.[9] Stimson identified 139 different domestic social and economic policy questions that had been asked in identical form in major public opinion polls in two or more years between 1956 and 1989. After determining the liberal–conservative direction of public responses to each question, he constructed a composite measure from the individual items. The resulting index measures the relative liberalism–conservatism of the public mood across a broad range of domestic policy concerns on a yearly basis for the period 1956–89.[10]

To consider the Dahl–Funston hypothesis that linkages between public opinion and Supreme Court decisions are mediated through the changing ideological composition of the Court, we again turned to existing measures. Specifically we used the ideology scores for individual justices constructed by Segal and Cover (1989) based on analyses of the justices' judicial philosophies at the time of their confirmation hearings.[11] To create an aggregate, yearly measure of the ideological composition of the Court, we summed Segal and Cover's individual ideology scores across the nine justices sitting on the Court in any year. A useful feature of this index is that it identifies the ideology of the justices based on their behavior before their respective appointments to Court, rather than on their voting records while on the Court (Sheehan, Mishler, and Songer 1992). This avoids the problem of circularity inherent in studies that attempt to explain Supreme Court decisions using measures based on those same decisions.

Finally, several additional measures were constructed to control for the possibility that the effects of public opinion on the Court are mediated through the effects of the president and Congress. Two aspects of the presidency were examined: the party of the president and a partisan-controlled measure of presidential popularity.[12] Two measures of the decision-making majority in Congress were also considered: the simple percentage of seats in the House of Representatives controlled by Democrats and a composite measure reflecting the presence and success of the conservative coalition in the House of Representatives.[13]

AN OVERVIEW OF BASIC TRENDS

Figure 1 compares the liberalism of the public mood and Supreme Court decisions for the period 1956–89.[14] Indicative of the face validity of our measure of Supreme Court decisions, the trends in the liberalism of the Court's decisions accord very well with what we think we know about recent history. Our measure indicates that the Court's decisions become increasingly liberal during the early Warren years, averaging a full standard deviation above the 33-year mean throughout the 1950s and early 1960s. The liberalism of the Court levels off in the mid-1960s, then falls precipitously across the Burger years, after which there is a continuing, but much more gradual and intermittent, increase in conservative decisions during the 1980s.

Also noteworthy in Figure 1 is the close correspondence between the trends in the Supreme Court decisions and the liberalism of the public mood across most of the period. Both series register increasing levels of liberalism during the 1950s and early 60s; both decline abruptly during the latter 1960s; and both reach bottom circa 1980, although they clearly diverge thereafter. Nevertheless, the simple correlation between the series (r = .56), though statistically significant, indicates that there are significant discrepancies in these trends, as well.

Part of the reason the correlation between the public mood and the Court's decisions is not stronger is that the strength and timing of their common trends clearly differ. These differences provide clues to the direction of causality. Figure 1 suggests that changes in the public mood generally precede the associated changes in the liberalism of Supreme Court decisions. For example, the increasing liberalism evident in both series in the late 1950s and early 1960s begins somewhat earlier in the public mood, as does the subsequent conservative tide observed in both series in the late 1960s and early 1970s. Al-ing that the average president in American history served for two terms and appointed two justices to the Court per term, he concluded that the Court was responsive because most presidents enjoyed ample opportunity "to tip the balance on a normally divided Court."

Elaborating this thesis, Funston (1975) demonstrated that the periods when the Court has been out of step with the dominant political coalition coincide with electoral realignments. In his view, the fact that these periods have been brief reflects the speed with which the dominant coalitions that emerge from realignments are able to appoint enough justices to alter the decision-making balance on the Court. Although Funston's data have been criticized (Canon and Ulmer 1976), the Dahl–Funston hypothesis regarding the primacy of membership change is widely accepted (see, e.g., Baum 1992; Schubert 1970; Segal 1985).

Notwithstanding the prominence of the Dahl–Funston hypothesis, another perspective holds that the Court can and does respond to public opinion even in the absence of membership change. The argument usually advanced is that Supreme Court justices are acutely aware of the limitations of the Court's power and its dependence on voluntary acquiescence to its decisions (see, e.g., Adamany and Grossman 1983). As Justice Frankfurter expresses it in *Baker* v. *Carr* (1962), "The Court's authority—possessed of neither the purse nor the sword—ultimately rests on sustained public confidence in its moral sanction."

According to this argument, the Court's concern for its authority makes it reluctant to depart too far or too long in its decisions from prevailing public sentiment. This is not to say that judges "play the role of Galahad." Neither is it to argue that justices consult opinion polls prior to making decisions or that they tailor their judicial philosophies to the latest fluctuations in public opinion on specific issues. It is rather to suggest both that members of the Court are political creatures, who are broadly aware of fundamental trends in ideological tenor of public opinion, and that at least some justices, consciously or not, may adjust their decisions at the margins to accommodate such fundamental trends.[3]

Even in the absence of concerns about the legitimacy of the Court's decisions, there are good reasons to believe that Supreme Court justices may be influenced by long-term changes in the climate of public opinion. Justices are no less susceptible than other individuals in society to influence by evolving societal norms and values. If, for example, attitudes in the political culture toward the role of women in society undergo significant changes over time, it is unlikely that the attitudes and beliefs of sitting justices can remain permanently immune to these changing mores. Gradually, the attitudes at least of some of the justices are likely to change, thus shifting the Court's ideological center of gravity, as well.

The likelihood, of course, is that under either of these variants of what we shall call the *political adjustment hypothesis*, the resulting changes in the decisions of individual justices will occur gradually over time and be almost imperceptible at the moment. Thus, it is unrealistic to expect the results of a specific public opinion poll to be linked to specific decisions either of an individual justice or of the Court as a whole. The expectation, rather, is that the overall pattern of justices' decisions—and thus the ideological center of gravity of the Court—should respond gradually, and at a considerable lag, to broad trends in the ideology of the public.

Little systematic evidence exists with respect to the political adjustment hypothesis, although anecdotes abound. A classic example, frequently cited, was the apparent retreat by the Supreme Court in the 1930s from its initial opposition to President Roosevelt's New Deal legislation (see, e.g., the citations in Caldeira 1987). Confronted by political pressures and by hostile public opinion mobilized in part by FDR in support of his "Court-packing" scheme, the Court abruptly reversed its string of anti–New Deal decisions upholding minimum-wage laws in *West Coast Hotel* v. *Parrish* (1937) and the Wagner Act in *NLRB* v. *Jones and Laughlin Steel Company* (1937), among other decisions. It did so, moreover, without any change in membership.

Also frequently cited is the Court's decision in *Koramatsu* v. *the United States* (1944), in which the popularly supported and presidentially ordered internment of Japanese–Americans was upheld by the Court despite widespread misgivings by constitutional scholars and jurists about the constitutionality of this decision. Even Chief Justice Rehnquist has acknowledged that the Court's decision in *Youngstown Sheet and Tube Company* v. *Sawyer* (1952) was probably influenced by popular sentiment toward President Truman and the Korean War.[4] Indeed, Rehnquist has argued:

> Judges, so long as they are relatively normal human beings, can no more escape being influenced by public opinion in the long run than can people working at other jobs. And, if a judge on coming to the bench were to decide to hermetically seal himself off from all manifestations of public opinion, he would accomplish very little; he would not be influenced by current public opinion, but instead would be influenced by the state of public opinion at the time he came to the bench (1986, 40–41).

Still further indicative of the importance of public opinion for the Court is the evidence that members of the Court may have at times attempted to manipulate public opinion. In an ingenious study drawing upon a series of early Gallup polls tracking public opinion across the period of the Court-packing fight, Caldeira (1987) demonstrates that the Court's precipitous change of direction on New Deal legislation played a critical role in undermining public support for FDR's Court-packing plan. Caldeira refrained from attributing political motives to the Court based on this evidence. However, his data, together with the anecdotes about other episodes in the Court's history, alliance" (p. 293). Dahl's thesis, of course, is a much elaborated and sophisticated version of the conventional wisdom succinctly expressed by Mr. Dooley in the opening epigram. Nevertheless, the Dahl–Dooley hypothesis, as Funston (1975) labeled it, has its critics.

In perhaps the most widely cited critique of this perspective, Casper (1976) argued that Dahl substantially underestimated the extent to which the Court exerts political influence independently of, and frequently in opposition to, law-making majorities. Criticizing the exclusive focus on cases in which the Court held federal legislation unconstitutional, Casper expanded Dahl's analysis to include state and local legislation nullified by the Court, as well as cases where the Court altered national policy by interpreting federal statutes creatively.[1] Based on this additional data, he concluded that the Court was substantially more active in protecting minorities—and more willing to alter or reinterpret the policies of the dominant law-making coalition—than Dahl's earlier analysis allowed.

Although the relationship of the Court's decisions to public opinion is central to this debate, none of the authors addresses that issue directly. Lacking direct evidence on public opinion because "scientific opinion polls are of relatively recent origin," Dahl used the position of the legislative majority in Congress as a surrogate for majority will (1957, 283). If the Court overturned a law recently enacted by Congress, he considered it to have countermanded the will of the dominant coalition and thus, indirectly, of majority public opinion. Funston (1975) and Casper (1976) used similar strategies.

Aided by the increasing availability of social surveys and public opinion polls, several recent studies (e.g., Barnum 1985; Page and Shapiro 1983) have examined the relationship between public opinion and the Court's decisions more directly, exploring the congruence between the Court's decisions in specific cases and public attitudes toward the central issues in those cases. In the most ambitious study of this type, Marshall (1989) compares specific Supreme Court rulings with national public opinion polls from the mid-1930s through the mid-1980s. He observes that "the modern Court has been an essentially majoritarian institution. Where clear polls margins exist, three-fifths to two-thirds of Court rulings reflect the polls" (p. 192). Marshall finds that the level of congruence varies by policy area but concludes that the Supreme Court, overall, was about as responsive to public opinion as were other policymaking institutions. (For a contrasting perspective based on a very different style of analysis, see G. Rosenberg 1991.)

Although generally supportive of the Dahl–Dooley hypothesis, congruence studies have their limitations as well. For one, congruence tells us very little about causal direction (Caldeira 1991). It cannot distinguish situations where the Court responds to public opinion from those where public opinion is shaped by the decisions of the Court.[2]

Compounding this problem is the tendency of public opinion studies to focus on the decisions of the Court only in a few landmark cases. Such cases are of obvious interest because of their political importance. Precisely for this reason, however, they are likely to be the cases in which the public is most interested and public opinion most intense. They also are likely to be the cases for which public opinion is most salient. As a result, these studies are limited in what they can tell us about the relationship of broader trends in public opinion and the myriad, more typical cases decided by the Court with little fanfare or publicity.

Another limitation of existing opinion studies identified by Barnum is that the questions asked in public opinion polls frequently do not correspond very closely with the specific issues in the cases with which they are matched (1985, 654). Of necessity, these studies must match specific decisions with broad opinions (e.g., comparing the limited right to abortion provided in *Roe* v. *Wade* with public responses to broad questions about support for abortion or even about the rights of woman).

The small number of Supreme Court cases in any issue area combined with the absence of extended times series data on public opinion in most policy areas also limits existing studies to relatively simple comparisons of the outcome of a single case with the level or direction of public opinion at the moment. This limits the introduction of statistical controls and prevents examination of the possibility that the impact of public opinion on the Court may occur at a significant lag.

More importantly, the focus on individual decisions and public opinion on specific policy issues at the moment assumes, in effect, that the impact of public opinion is direct and immediate—that the decisions of justices on pending cases are influenced by relevant public opinion polls seen in the morning paper. A more realistic assumption is that public opinion, if it is important, influences the Court as a result of gradual, almost imperceptible changes in the attitudes and beliefs of individual justices as they adapt, consciously or not, to long-term, fundamental trends in the ideological temper of the public. Examination of the later possibility requires extended time series data, previously unavailable, on ideological trends both in public opinion and Supreme Court decisions.

In short, the nature and extent of the relationship between public opinion and Supreme Court decisions remains very much in question.

PATHWAYS OF INFLUENCE

As to the processes or mechanisms by which public opinion might influence the Supreme Court, Dahl attributed the Court's reluctance to invalidate majoritarian policies to the president's power of judicial appointment (1957, 284). He argued that it would be "naive to assume that the Supreme Court either would or could play the role of Galahad," modifying its positions to accommodate public opinion. Reason-

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THE SUPREME COURT AS A COUNTERMAJORITARIAN INSTITUTION? THE IMPACT OF PUBLIC OPINION ON SUPREME COURT DECISIONS

WILLIAM MISHLER *University of South Carolina*
REGINALD S. SHEEHAN *Michigan State University*

Although normative questions about the role of the Supreme Court as a countermajoritarian institution have long excited controversy in democratic theory, empirical questions about how far the Court acts contrary to majoritarian opinion have received less attention. Time series analyses for the period 1956–89 indicate the existence of a reciprocal and positive relationship between long-term trends in aggregate public opinion and the Court's collective decisions. The Court's ideological composition changes in response to previous shifts in the partisan and ideological orientation of the president and Congress. The Court also responds to public opinion at the margins even in the absence of membership change. Since 1981, the relationship has vanished or turned negative in direction. The Court's ideological balance has been upset by an unbroken string of conservative-to-moderate appointments, thereby undermining the dynamics that promote judicial responsiveness and raising questions about the majoritarianism of the contemporary and future Court.

> No matter whether th' constitution follows th' flag or not, th' supreme coort follows th' ilicition returns.
> —(Mr. Dooley [Finley Peter Dunne])

The role of the Supreme Court in national policymaking has long excited controversy in American democratic theory. Little argument exists about the role played by the Court; constitutional and judicial scholars agree that the Court has considerable policymaking influence. The controversy, rather, surrounds the seeming incongruity of an unelected and therefore "undemocratic" institution exercising significant political power in a democratic society. Bickel (1962) refers to this as the "countermajoritarian difficulty" of the Court. He argues that declaring actions unconstitutional that were initiated by duly elected representatives vitiates the will of the people. Although Bickel refers specifically to the power of judicial review, his critique can be extended to the exercise of other judicial powers in the policymaking process as well.

The rationale traditionally advanced for investing substantial political power in an unelected Court is the protection of minorities from democratic excess. The line between popular sovereignty and majority tyranny has proven notoriously difficult to draw. Nevertheless the assumption has been that the Court functions, at least to a degree, independent of majority opinion or the popular mood.

Although the traditional view of the Court as a countermajoritarian institution has always been controversial on normative grounds, the empirical validity of this perspective is subject to debate, as well. In a celebrated series of articles beginning with Dahl's (1957) classic essay, judicial scholars have engaged in a continuing and increasingly rigorous debate about the extent to which the Court is, in fact, insulated from majoritarian pressures.

We here join that debate. Specifically, we use time series methods and aggregate data to examine the relationship between public opinion and the decisions of the Supreme Court during the period 1956–89. In addition to measuring the strength of the relationship between public opinion and the Court's decisions and attempting to assess its causal direction, we develop and test alternative hypotheses about the pathways of public influence on the Court. Of particular interest is whether the Court attends directly to shifts in public opinion or responds only indirectly to shifting tides of public opinion through politically motivated changes in the membership of the Court.

PUBLIC OPINION AND THE SUPREME COURT

Among the first to challenge the empirical validity of the traditional view of the Supreme Court as a countermajoritarian institution, Dahl argued, "The view of the Court as protector of the liberties of minorities against the tyranny of majorities is beset with . . . difficulties that are not so much ideological as matters of fact and logic" (1957, 283). Observing that the Court only rarely overturned recently enacted policies of extant law-making majorities in Congress and the White House and almost never to protect the interests of identifiable minorities, Dahl concluded that American politics is characterized by relatively stable and enduring electoral/political coalitions that dominate national policymaking. Moreover, "Except for short-lived transitional periods when the old alliance is disintegrating and the new one struggling to take control of political institutions, the Supreme Court is inevitably a part of the dominant national alliance. As an element in the political leadership the Court inevitably supports the major policies of the

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James Johnson is Assistant Professor of Political Science, University of Rochester, Rochester, NY 14627.

domizing strategies precisely for the purpose "of expunging from the game all details except the mathematical structure of the payoff, and from players all communicative relations" (Schelling 1960, 105, 165).

35. The relevant actors might revert to strategic interaction and risk indeterminacy, or they might break off relations altogether (Habermas 1984, 17–18; idem 1979, 3–4).

36. For an interesting move in this direction, see Bohman 1988.

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the *ordering condition* for ϵ (with respect to μ) if, for every three distinct candidates i, j, and h, if $S_i(\mu) < S_j(\mu)$, then $p_{ih} \le \epsilon \cdot p_{jh}$.

We are at last prepared to define our central concept. An election result μ is a *voting equilibrium* if and only if, for every positive number ϵ, there exists some vector p of positive pivot probabilities that justifies μ and that satisfies the ordering condition for ϵ; that is, an election result μ is a voting equilibrium if the voters would be willing to cast their votes in accordance with μ, while holding perceptions that candidates with low predicted scores under μ have very small chances of being serious contenders for victory relative to candidates with higher predicted scores under μ.

Our first basic result is that voting equilibria always exist. The proof will be given shortly.

THEOREM 1. *In every electoral situation, the set of voting equilibria is nonempty.*

Demonstrating that an election result μ is a voting equilibrium requires the construction of an infinite sequence of voter perceptions that justify μ and satisfy the ordering condition for ever smaller values of ϵ. However, we can provide a simple limit condition that greatly facilitates the search for voting equilibria.

Given any voting equilibrium μ, select for each positive number ϵ a pivot probability vector $p(\epsilon)$ that justifies μ and satisfies the ordering condition for ϵ. Typically, the voters will perceive the pivot probabilities to be very small. However, the ordering condition is homogeneous in p; that is, multiplying all of the components of $p(\epsilon)$ by the same positive constant yields another vector that still satisfies the ordering condition for ϵ. Furthermore, as already noted, the resulting vector will still justify μ. In particular, we can rescale each vector $p(\epsilon)$ to make its components sum to 1, that is, to make each $p(\epsilon)$ a probability distribution over H. The set of probability distributions over H is a closed and bounded set; hence, there will exist a subsequence of the rescaled $p(\epsilon)$-vectors that converages, as ϵ goes to zero, to some vector q that is also a probability distribution over H. Since the expected gain function G is continuous, q will justify μ. This limiting q will also satisfy the ordering conditions for all positive ϵ; therefore, a pair of candidates ij must have pivot probability q_{ij} equal to zero if there exists some other candidate h with a predicted score higher than at least one of them (e.g., if $S_i(\mu) < S_h(\mu)$, then $q_{ij} \le \epsilon \cdot q_{hj}$ for all positive ϵ). This gives us the following necessary condition for a voting equilibrium.

THEOREM 2. *If μ is a voting equilibrium, then there exists some probability distribution $q = (q_{ij})_{ij\in H}$ that justifies μ and for which, whenever $q_{ij} > 0$, $S_i(\mu) = \max_{h\neq j} S_h(\mu)$ and $S_j(\mu) = \max_{h\neq i} S_h(\mu)$.*

We say that a probability vector q *supports* the voting equilibrium μ if it satisfies the conditions in Theorem 2 for μ. If q supports a voting equilibrium μ, then each component q_{ij} can be interpreted as the perceived conditional probability of a tie for first place between candidates i and j, given that some pair of candidates is tied for first place and given that very accurate preelection polls predicted the election result μ. The conditions in the theorem imply that q_{ij} can be positive only if one of two conditions is satisfied: either candidates i and j are both in the set of likely winners, or one of these two candidates is the unique likely winner, and the other candidate has the second-highest predicted score (i.e., only if each is a relatively "serious threat" to the other).

Proof of Theorem 1. The proof employs a straightforward fixed-point argument and is similar to the existence proof for proper equilibria of noncooperative games (see Myerson 1978). Take any number ϵ such that $0 < \epsilon < 1$. For any election result μ, let $Q^\epsilon(\mu)$ be the set of all pivot probability vectors p that satisfy the ordering condition for ϵ and for which $p_{ij} \ge \epsilon^{k^2}/k^2$, for all $ij \in H$. It is easily shown that $Q^\epsilon(\cdot)$ maps every μ into a nonempty convex set and is upper-hemicontinuous in μ.[6] Similarly, the voter response mapping $R(\cdot)$ maps every pivot probability vector p into a nonempty convex set of election results and is upper-hemicontinuous in p. Thus, by the Kakutani fixed-point theorem, there exists a pair $(p^\epsilon, \mu^\epsilon) \in Q^\epsilon(\mu^\epsilon) \times R(p^\epsilon)$. Now consider a sequence of values for ϵ that converges to zero. Since the sets $V \times T$ and H are both finite, there must be an infinite subsequence for which the sets

$$\{(v, u) \in V \times T \,|\, \mu^\epsilon(v, u) > 0\} \quad \text{and}$$

$$\{ij \in H \,|\, S_i(\mu^\epsilon) < S_j(\mu^\epsilon)\}$$

are constant for all ϵ in the subsequence. Let $\mu = \mu^{\epsilon'}$ for any particular ϵ' in this subsequence. Then for every ϵ in the subsequence, p^ϵ justifies μ (since sets of the first type are constant along the subsequence) and satisfies the ordering condition for ϵ (since sets of the second type are constant). Hence, μ is a voting equilibrium. Q.E.D.

EXAMPLE 1: COMPARISON OF VOTING SYSTEMS

We are now prepared to examine how, for a fixed electorate, changes in the voting system can affect equilibrium predictions of election results. In our first example, there are three candidates and three types of voters whose utility vectors, together with the distribution of types within the electorate, are given in the following table.

Voter Type	Utility Vector	Proportion of Electorate
A	$u^A = (10, 9, 0)$	$f(u^A) = .3$
B	$u^B = (9, 10, 0)$	$f(u^B) = .3$
C	$u^C = (0, 0, 10)$	$f(u^C) = .4$

To interpret these parameters, suppose that candidates 1 and 2 are leftist candidates, and candidate 3 a rightist. Then 60% of the voters are leftists who greatly prefer both candidates 1 and 2 over candidate 3. Of these leftist voters, however, half (30% of the electorate) are A-type voters, who have a slight preference for candidate 1; and the other half are B-type voters, who have a slight preference for candidate 2. The C-type voters are rightists, who strictly prefer candidate 3 over both of the leftist candidates and are indifferent between the two leftists. These C-type voters form 40% of the electorate. This example is similar to that discussed by Borda (1781). It also models a situation which has arisen on numerous occasions in two-party systems, when the nominee of the party holding majority support is confronted with a third, independent candidate contesting that support. The 1912 U.S. presidential election is an oft-cited instance of this situation.

Under plurality rule, there are three voting equilibria. At one equilibrium, all of the leftists coordinate their support behind candidate 1, voting (1, 0, 0); and candidate 1 beats candidate 3. In our notation, this equilibrium is written as

$$\mu((1,0,0),u^A)=.3,\ \mu((1,0,0),u^B)=.3,$$

$$\mu((0,0,1),u^C)=.4;$$

that is, 30% of the voters are A-type leftists voting for candidate 1, 30% are B-type leftists also voting for candidate 1, and 40% are C-type rightists voting for candidate 3. The election result yields predicted scores (expected per capita vote totals) of $S_1(\mu) = .6$, $S_2(\mu) = 0$, $S_3(\mu) = .4$. Thus, candidate 1 is the only likely winner, i.e., $W(\mu) = \{1\}$. This equilibrium is supported by the pivot probability vector q satisfying $(q_{12}, q_{13}, q_{23}) = (0, 1, 0)$; that is, if there were a close race for first place, the voters would expect (with conditional probability very near 1) this close race to be between candidates 1 and 3, who have the top two predicted scores. Fearing a close race between candidates 1 and 3, the B-type leftists vote for candidate 1 even though they slightly prefer candidate 2. (To a B-type leftist with perceptions p, the prospective rating of candidate 1 is $9p_{13} - p_{12}$; and that of candidate 2 is $p_{12} + 10p_{23}$. For every p proportionally near q, the first of these quantities is larger than the second.)

There is a second voting equilibrium under the plurality rule, in which the leftist voters, instead, coordinate behind candidate 2 to beat candidate 3. This equilibrium is

$$\mu((0,1,0),u^A)=.3,\ \mu((0,1,0),u^B)=.3,$$

$$\mu((0,0,1),u^C)=.4.$$

The predicted scores and likely winners are then

$$S_1(\mu)=0,\ S_2(\mu)=.6,\ S_3(\mu)=.4,\text{ and }W(\mu)=\{2\}.$$

This equilibrium is supported by the pivot probability vector $(q_{12}, q_{13}, q_{23}) = (0, 0, 1)$.

There is a third plurality rule equilibrium in which the leftists split their votes between candidates 1 and 2 and candidate 3 is the sole likely winner. This equilibrium is

$$\mu((1,0,0),u^A)=.3,\ \mu((0,1,0),u^B)=.3,$$

$$\mu((0,0,1),u^C)=.4,$$

with

$$S_1(\mu)=.3,\ S_2(\mu)=.3,\ S_3(\mu)=.4,\text{ and }W(\mu)=\{3\}.$$

This equilibrium is supported by any pivot probability vector q such that $q_{12} = 0$, $9/19 \le q_{13} \le 10/19$, $q_{23} = 1 - q_{13}$. Here, since candidates 1 and 2 are perceived to have comparable likelihoods of being in contention for victory with candidate 3, the leftist voters fail to coordinate, so that candidate 3 is the likely winner.

This third equilibrium violates Duverger's Law, which asserts that only two candidates should be expected to get positive scores under plurality rule (see Riker 1982). The existence of such a non-Duverger equilibrium is robust to small changes in the parameters. For example, if the proportion of A-type voters were slightly larger than the proportion of B-type voters, say, $f(u^A) = .31$ and $f(u^B) = .29$, then there would still exist a non-Duverger equilibrium in which candidate 1's chances of seriously challenging candidate 3 were perceived to be somewhat worse than candidate 2's chances, leaving the A-type voters indifferent between voting for 1 and for 2. This equilibrium is

$$\mu((1,0,0),u^A)=.30,\ \mu((0,1,0),u^A)=.01$$

$$\mu((0,1,0),u^B)=.29,\ \mu((0,0,1),u^C)=.4,$$

with

$$S_1(\mu)=.3,\ S_2(\mu)=.3,\ S_3(\mu)=.4,\text{ and }W(\mu)=\{3\}.$$

It is supported by $q_{12} = 0$, $q_{13} = 9/19$, $q_{23} = 10/19$. Thus, Duverger's Law cannot be derived exclusively from analyses of voting equilibria. (This result suggests that some of the interpretive discussion in Palfrey 1989 should be reconsidered.) Any derivation of Duverger's Law would seem to require some additional assumption of dynamic stability or persistence (see, e.g., Kalai and Samet 1984), to eliminate equilibria of the type just illustrated.

Under approval voting, there are three voting equilibria, in none of which is candidate 3 the only likely winner. In one equilibrium, candidate 1 is the only likely winner:

$$\mu((1,0,0),u^A)=.3,\ \mu((1,1,0),u^B)=.3,$$

$$\mu((0,0,1),u^C)=.4,$$

with

$$S_1(\mu)=.6,\ S_2(\mu)=.3,\ S_3(\mu)=.4,\text{ and }W(\mu)=\{1\}.$$

This equilibrium is supported by $(q_{12}, q_{13}, q_{23}) = (0, 1, 0)$. The voters expect that if any two candidates are involved in a close race for first place, then they will almost surely be candidates 1 and 3. Hence, all B-type voters double-vote for candidates 1 and 2. However, voters also consider a close race for first place between candidates 1 and 2 (with predicted scores of .6 and .3), while much less likely than a close race between candidates 1 and 3, to be much more likely than a close race between candidates 2 and 3 (with predicted scores of .3 and .4). Therefore, A-type voters only cast single votes for candidate 1.

Other election results are "supported" by this q (in the sense that the conditions of Theorem 2 are satisfied) but fail to be equilibria because perceptions satisfying the ordering condition for sufficiently small ϵ do not exist. Consider, for example, the election result

$$\mu((1, 0, 0), u^A) = .25,\ \mu((1, 1, 0), u^A) = .05,$$

$$\mu((1, 1, 0), u^B) = .3,\ \mu((0, 0, 1), u^C) = .4,$$

with

$$S_1(\mu) = .6,\ S_2(\mu) = .35,\ S_3(\mu) = .4, \text{ and } W(\mu) = \{1\},$$

in which some A-type voters double-vote. This result can be justified only by positive pivot probabilities (p_{12}, p_{13}, p_{23}) satisfying $p_{12}/p_{13} \le 1/9$ and $p_{23}/p_{12} = 1/9$. (The inequality ensures that B-type voters double-vote, and the equation leaves A-type voters indifferent between casting single or double votes by making their prospective rating of candidate 2 equal to 0.). However, the ordering condition for ϵ requires that we be able to satisfy both $p_{12}/p_{13} \le \epsilon$ and $p_{23}/p_{12} \le \epsilon$; and the latter inequality cannot be satisfied (by perceptions justifying μ) for values of ϵ smaller than 1/9.

There is another equilibrium, in which candidate 2 is the only likely winner:

$$\mu((1, 1, 0), u^A) = .3,\ \mu((0, 1, 0), u^B) = .3,$$

$$\mu((0, 0, 1), u^C) = .4,$$

with

$$S_1(\mu) = .3,\ S_2(\mu) = .6,\ S_3(\mu) = .4, \text{ and } W(\mu) = \{2\},$$

supported by $(q_{12}, q_{13}, q_{23}) = (0, 0, 1)$.

In order to find the third equilibrium under approval voting, let us seek an equilibrium in which (as in the non-Duverger equilibrium in the plurality rule) candidates 1 and 2 are considered to have equal chances of being in a close race with candidate 3; that is, let us suppose that the pivot probability vector supporting the equilibrium is $q_{13} = q_{23} = \beta$, $q_{12} = 1 - 2\beta$, with $0 \le \beta \le 1/2$.

If β were relatively large, say $\beta = 1/3$ (so that all close two-way races for victory were perceived to be equally likely), then

$$G(q, v, u^A) = (1/3)(v_1 - v_2) \cdot (10 - 9) + (1/3)(v_1 - v_3)$$

$$\cdot (10 - 0) + (1/3)(v_2 - v_3) \cdot (9 - 0)$$

$$= (11/3)v_1 + (8/3)v_2 - (19/3)v_3;$$

and the A-type voters would vote for both candidates 1 and 2. Similarly, the B-type voters would also vote for both leftist candidates; and the predicted scores would be .6 votes per capita for candidates 1 and 2, but only .4 votes per capita for candidate 3. Thus, the set of likely winners would be {1, 2}, and the conditions of Theorem 2 would require that $q_{12} = 1$ (i.e., $\beta = 0$).

If β were quite small, say $\beta = 0$ (so that the only serious race was perceived to be between candidates 1 and 2), then $G(q, v, u^A) = v_1 - v_2$. The A-type voters would only vote for candidate 1, the B-type voters would similarly vote only for candidate 2, and the predicted scores would be .3 for each of the first two candidates and .4 for the rightist candidate 3. The conditions of Theorem 2 would require that $q_{12} = 0$ (i.e., $\beta = 1/2$), because neither candidate 1 nor candidate 2 would be a likely winner.

To construct an approval voting equilibrium in which candidates 1 and 2 are treated symmetrically, we cannot have the leftist voters all single-voting for their most preferred candidate; nor can they all be double-voting. Generally, if $(q_{12}, q_{13}, q_{23}) = (1 - 2\beta, \beta, \beta)$, we have

$$G(q, v, u^A) = (1 - 2\beta)(v_1 - v_2)1 + \beta(v_1 - v_3)10$$

$$+ \beta(v_2 - v_3)9$$

$$= (1 + 8\beta)v_1 + (11\beta - 1)v_2 - (19\beta)v_3.$$

When $\beta = 1/11$, the leftist voters (both A- and B-types) are indifferent between voting only for their most preferred candidate and double-voting for both leftist candidates. Then, $q_{12} = 9/11$ and $q_{13} = q_{23} = 1/11$, and we obtain our third equilibrium, in which

$$\mu((1, 0, 0), u^A) = .2,\ \mu((1, 1, 0), u^A) = .1,$$

$$\mu((0, 1, 0), u^B) = .2,\ \mu((1, 1, 0), u^B) = .1,$$

$$\mu((0, 0, 1), u^C) = .4,$$

with

$$S_1(\mu) = S_2(\mu) = S_3(\mu) = .4, \text{ and } W(\mu) = \{1, 2, 3\}.$$

Here, a third of the leftist voters double-vote for both leftist candidates, while two-thirds of the leftist voters single-vote only for their most preferred candidate, so that the expected per capita vote totals of all three candidates are equal to .4. In this equilibrium, the race between each pair of candidates is taken seriously; but a close race between candidates 1 and 2 is perceived to be nine times more likely than a close race between either 2 and 3 or 1 and 3. We shall return to this example, and illustrate by a limit analysis how equal predicted scores for the three candidates can be consistent with unequal pivot probabilities for the three candidate pairs. (Short of the limit, the predicted scores of candidates 1 and 2 will be slightly higher than the predicted score of candidate 3.)

Under the Borda rule, there is a set of equilibria, all with the same predicted outcome in which all three candidates are likely winners. The equilibria are of the form

$$\mu((2,1,0),u^A)=\alpha, \qquad \mu((2,0,1),u^A)=.3-\alpha$$

$$\mu((1,2,0),u^B)=.4-\alpha, \qquad \mu((0,2,1),u^B)=\alpha-.1$$

$$\mu((0,1,2),u^C)=.4-\alpha, \qquad \mu((1,0,2),u^C)=\alpha,$$

where $.1 \leq \alpha \leq .3$. For every Borda rule equilibrium μ, we have

$$S_1(\mu)=S_2(\mu)=S_3(\mu)=1, \text{ and } W(\mu)=\{1,2,3\}.$$

These equilibria are supported by $q_{12} = 28/30$, $q_{13} = q_{23} = 1/30$.

All races are taken seriously, but candidate 3 is perceived to be only a slight threat to the other two candidates. (If he were perceived to be a very likely threat, the A- and B-type voters would all cast zero votes for him, and the threat would disappear. But if he were perceived to be no threat at all, every leftist would "dump" his 1-vote on candidate 3, in order to maximally differentiate between the more- and less-preferred of candidates 1 and 2. Consequently, candidate 3 would win.)

In short, for the electoral situation studied here, we find that under the plurality rule, there is an equilibrium in which the minority (rightist) candidate is the only likely winner. Under the Borda rule, the minority candidate is always a likely winner at equilibrium. Only under approval voting do we see simultaneously the existence of equilibria in which the minority candidate is not a likely winner and the nonexistence of equilibria in which the minority candidate is the only likely winner.

EXAMPLE 2: THE EFFECT OF A SMALL MINORITY

We now consider a second example, which differs from the first only in that the number of supporters of candidate 3 is reduced to 2% of the population, while the remaining 98% of the voters are evenly divided between A-type voters who prefer candidate 1 and B-type voters who prefer candidate 2:

Voter Type	Utility Vector	Proportion of Electorate
A	$u^A = (10, 9, 0)$	$f(u^A) = .49$
B	$u^B = (9, 10, 0)$	$f(u^B) = .49$
C	$u^C = (0, 0, 10)$	$f(u^C) = .02$

Under the Borda rule, there are still no equilibria in which candidate 3 is not a likely winner. All equilibria are of the form

$$\mu((2,1,0),u^A)=\alpha, \qquad \mu((2,0,1),u^A)=.49-\alpha$$

$$\mu((1,2,0),u^B)=.02-\alpha, \qquad \mu((0,2,1),u^B)=.47+\alpha$$

$$\mu((0,1,2),u^C)=.02-\alpha, \qquad \mu((1,0,2),u^C)=\alpha,$$

where $0 \leq \alpha \leq .02$. As in Example 1, each equilibrium μ has

$$S_1(\mu)=S_2(\mu)=S_3(\mu)=1 \text{ and } W(\mu)=\{1,2,3\}$$

and is supported by $q_{12} = 28/30$, $q_{13} = q_{23} = 1/30$. Even though there are relatively few voters who prefer candidate 3, there are no Borda rule equilibria in which he is not a likely winner. If q_{12} were greater than 28/30, while $q_{13} = q_{23}$, then the A- and B-type voters would want to maximally separate candidates 1 and 2—voting (2, 0, 1) and (0, 2, 1), respectively—and candidate 3 would win with a per capita vote total of $.98 \cdot 1 + .02 \cdot 2 = 1.02$.

Under approval voting, there is a unique voting equilibrium μ in which everyone votes only for his most preferred candidate:

$$\mu((1,0,0),u^A)=.49, \; \mu((0,1,0),u^B)=.49,$$

$$\mu((0,0,1),u^C)=.02.$$

Thus, $S_1(\mu) = S_2(\mu) = .49$, $S_3(\mu) = .02$, and $W(\mu) = \{1, 2\}$. This equilibrium is supported by $q_{12} = 1$, and $q_{13} = q_{23} = 0$.

Even though everyone single-votes in the unique approval voting equilibrium for this example, the ability of the voters to cast votes for more than one candidate does have an impact: the set of equilibria under plurality rule is larger than the set of equilibria under approval voting. For example, there is also a plurality rule equilibrium μ in which

$$\mu((1,0,0),u^A)=.49, \; \mu((1,0,0),u^B)=.49,$$

$$\mu((0,0,1),u^C)=.02,$$

with

$$S_1(\mu)=.98, \; S_2(\mu)=0, \; S_3(\mu)=.02, \text{ and } W(\mu)=\{1\}.$$

This equilibrium is supported by $(q_{12}, q_{13}, q_{23}) = (0, 1, 0)$. (This election result would not be an equilibrium under approval voting, because the B-type voters would choose to give second approval votes to candidate 2, and candidate 2's predicted score would then be higher than candidate 3's, invalidating the perception that a race between candidates 1 and 2 for first place is much less likely than a race between candidates 1 and 3.) Similarly, there is a plurality rule equilibrium in which all A- and B-type voters vote for candidate 2, who is then the only likely winner. However, candidate 3 is not a likely winner in any equilibrium under plurality rule.

LIMITS OF NASH EQUILIBRIA

We return to Example 1, in which 60% of the voters are A- or B-type "leftists." Recall that under approval voting, there exists an equilibrium μ in which two-thirds of the leftists (of each type) vote only for their most preferred candidate, and one-third of the leftists double-vote for candidates 1 and 2; that is,

$$\mu((1, 0, 0), u^A) = .2, \mu((1, 1, 0), u^A) = .1$$

$$\mu((0, 1, 0), u^B) = .2, \mu((1, 1, 0), u^B) = .1$$

$$\mu((0, 0, 1), u^C) = .4.$$

This equilibrium is supported by the pivot probabilities $q_{12} = 9/11$ and $q_{13} = 1/11 = q_{23}$, and $S_1(\mu) = S_2(\mu) = S_3(\mu) = .4$, so that all three candidates have the same per capita scores. The conditions of Theorem 2 are satisfied by q and μ, because all three candidates are likely winners.

If all three candidates have the same predicted scores, how is it that the conditional tie probabilities q_{ij} are not the same for all pairs of candidates? A naive interpretation of our model might suggest that all pivot probabilities should be perceived to be equal if all candidates have the same predicted score. However, this interpretation overlooks the fact that our model is intended to be a limiting approximation to elections involving large finite electorates.

To see how such an equilibrium can arise as the limit of Nash equilibria for large finite electorates, consider a voting "game" in which there is a large number M of voters, of whom .3M are type u^A, .3M are type u^B, and .4M are type u^C. These voters will select a candidate by approval voting. This voting game has a randomized Nash equilibrium in which each leftist (A- or B-type) voter independently randomizes between voting only for his most preferred candidate, with probability $1 - \rho$, and double-voting for both candidates 1 and 2, with probability ρ, while each C-type voter votes only for candidate 3. The parameter ρ depends on M according to the asymptotic formula $\rho \approx 1/3 + .9476/\sqrt{M}$. For example, suppose that M = 1,000,000. Then $\rho = .33428$, and the vote totals for candidates 1 and 2 are independent random variables, approximately normally distributed with mean 400,284 and standard deviation 258. The vote total for candidate 3 is precisely 400,000. At the Nash equilibrium, the probability that candidates 1 and 2 are tied ahead of candidate 3 is $p_{12} = .001027$, whereas the probability that candidates 1 and 3 are tied ahead of candidate 2 is $p_{13} = .000114$. Hence, $p_{12}/p_{13} = 9$. Similarly, $p_{12}/p_{23} = 9$. These pivot probabilities correspond to a normalized probability distribution q such that $q_{13} = q_{23} = 1/11$ and $q_{12} = 9/11$; that is, conditional on some pair of candidates' being tied for first place, the probability that the tie is between candidates 1 and 2 is 9/11. The candidates' predicted scores are all very close to .4 (actually, .400284, .400284, and .4); and the standard deviations of the scores are .000258, .000258, and 0.

Notice that uncertainty about a candidate's vote total may be large relative to the size of an individual's vote (the standard deviation of 258 is much bigger than 1), even though the uncertainty about the per capita vote totals is very small. Thus, when the electorate is large, near equality of the predicted scores for different candidates does not necessarily mean that there is a large probability of a tie between them.

We do not offer here an equivalence theorem between the set of limits of Nash equilibria of large voting games (as the number of voters goes to infinity) and the set of voting equilibria, as defined here. Such a result would depend on assumptions about how the large finite voting games are specified. For example, the number of voters of each type may be either deterministic or random, and we may or may not admit the possibility of a vanishingly small fraction of the electorate that has preferences outside of the given type set T. We suspect that, for many specifications, the set of limits of Nash equilibria may turn out to be somewhat larger than the set of ordered voting equilibria. It seems reasonable to expect that a limit of Nash equilibria should be in R(q) for some pivot probability vector q, but it may be that this vector q is not the limit of perceptions satisfying the ordering condition. Future research should consider other (weaker) restrictions on perceptions. However, we believe that the ordering condition has some intuitive appeal and that voters would be much less likely to act in accordance with Nash equilibria that violated that condition, should such Nash equilibria exist.[7]

A CANDIDATE-POSITIONING GAME

In the preceding analysis, we assumed that the voters' preferences over the candidates were exogenously given. However, real voters' preferences over candidates typically depend on the candidates' positions on the policy choices that confront the government; and candidates may choose their positions (at least, in part) with the objective of becoming likely winners. Changes in the electoral system may change the way that candidates position themselves on the issues. To investigate this question, we consider a candidate positioning game in the tradition of Hotelling (1929) and Downs (1957).

Suppose that the policy space is [0, 100], the set of all real numbers between 0 and 100 (representing, say, the percentage tariff to be imposed on imported goods); and suppose that there are three candidates, numbered 1, 2, and 3. Each candidate must publicly choose a point in policy space that represents the policy he promises to implement if elected (say, the tariff that he will impose). Each voter has a utility function that is defined on the policy space, and his utilities for the candidates are determined by the positions that the candidates take. To be specific, we suppose that each voter has a most preferred point θ between 0 and 100; and his utility for candidate i, if the candidate takes position x, is $u_i(\theta) = -(x - \theta)^2$. The voters' ideal points are uniformly distributed over the set {0, 1, 2, . . . , 100}, with 1/101 of the voters having each ideal point in this set. If x_i denotes the position chosen by candidate i, then the set of utility types will be

$$T = \{(-(x_1 - \theta)^2,\ -(x_2 - \theta)^2,\ -(x_3 - \theta)^2) \mid \theta \in \{0, 1, 2, \ldots, 100\}\},$$

with $f(u) = 1/101$ for each vector u in T.

Under any given electoral system V, for any vector $x = (x_1, x_2, x_3)$, we let E(x) denote the set of voting equilibria for the electoral situation that would exist after the candidates chose the positions listed in x. A pair (x, μ) is a *positional equilibrium* under V if and only if it satisfies the following three conditions:

1. μ is in E(x).
2. For each candidate i in $W(\mu)$, for every position y_i, there exists a voting equilibrium η in $E(x_{-i}, y_i)$ such that either $W(\mu) \subseteq W(\eta)$ or $i \notin W(\eta)$.
3. For each candidate j who is not in $W(\mu)$, for every position y_j, there exists a voting equilibrium η in $E(x_{-j}, y_j)$ such that $j \notin W(\eta)$.

Here, (x_{-j}, y_j) denotes the vector that arises from x when the j^{th} component is changed from x_j to y_j. Condition 1 asserts that μ is a voting equilibrium in the election which results when the candidates choose the positions indicated by x, condition 2 asserts that a change of position by a likely winner can lead to either a new equilibrium in which no other likely winners are eliminated or a new equilibrium in which he himself is eliminated, and condition 3 asserts that a change in position by a candidate who is not a likely winner can lead to a new equilibrium in which he is still not a likely winner.[8]

In the context of this definition, we can examine how different electoral systems differ in their implications for candidate positioning. The following two theorems assert that the plurality rule and approval voting have very different implications. A candidate at almost any position can win under the plurality rule, while only candidates at the median position can win under approval voting.

THEOREM 3. *Under plurality rule, for any number z strictly between 0 and 100, there exists a positional equilibrium such that one candidate takes the position z and is the only likely winner.*

THEOREM 4. *Under approval voting, in any positional equilibrium, all likely winners take the position 50.*

Proof of Theorem 3. The definition of a positional equilibrium allows us to deter candidates from changing their positions by selecting among the resulting voting equilibria. The key to the proof is the existence of many voting equilibria under plurality rule. To simplify the proof, we consider here only the case where $0 < z \leq 50$. (An analogous argument can be used to cover the case where z is between 50 and 100.) Let $x_1 = z$, $x_2 = 100 - z/2$, and $x_3 = 50$. Then there exists a voting equilibrium μ in which all voters with ideal points between 0 and $50 + z/4$ vote for candidate 1, all voters with ideal points between $50 + z/4$ and 100 vote for candidate 2, and no voters vote for candidate 3. This voting equilibrium is supported by letting $q_{12} = 1$ and $q_{13} = q_{23} = 0$. Perceiving the only serious race to be between candidates 1 and 2, each voter votes for his more preferred of these two candidates. Because candidate 1 is closer to the median than candidate 2, candidate 1 will have the highest predicted score and thus will be the only likely winner. The perception that candidate 3 is not a serious contender is justified at equilibrium because candidate 3 gets no votes.

To verify that (x, μ) is a positional equilibrium, we must show that candidate 2 cannot gain by choosing a position closer to the median. If candidate 2 "relocates" between z and $100 - z$, then a majority of the voters will prefer candidate 2 to candidate 1. But this does not mean that candidate 2 will necessarily win the election, because there will be other voting equilibria supported by $q_{13} = 1$ in the new situation. We can deter candidate 2 from shifting his position by specifying that if candidate 2 moves to any position outside of the interval $[100 - z/2, 100]$, all voters will behave according to the voting equilibrium in which only the race between candidates 1 and 3 is taken seriously. (In the real political arena, this switch in public perceptions could result from some political pundit's calling candidate 2 an "opportunist" if he chooses a position short of $100 - z/2$.)

Given our specifications, candidate 1 is the unique likely winner (i.e., condition 2 is satisfied). Candidate 2 cannot become a likely winner by changing his position. And candidate 3 cannot make himself a likely winner by moving away from 50 if the voters continue to focus on the equilibrium of the resulting situation in which only the race between candidates 1 and 2 is taken seriously (i.e., condition 3 is satisfied).[9]

Q.E.D.

In the proof of Theorem 3, we found a voting equilibrium in which candidate 3 chose a position at the median point 50 and candidates 1 and 2 chose other positions away from 50 but the only serious race was perceived to be between candidates 1 and 2. Under the plurality rule, a voter who voted for candidate 3 would have to forfeit any chance of influencing the serious race between candidates 1 and 2; thus, the perception that candidate 3 was not a serious contender destroyed his ability to get votes. Under approval voting, however, the median candidate cannot be so easily defeated by such a perception, because a voter can vote for candidate 3 without forfeiting his ability also to vote for the candidate that he prefers among candidates 1 and 2. In fact, as we shall show in the proof of the Lemma, a candidate at 50 will always get approval votes from more than half the voters under any election result that can be justified by positive pivot probabilities.

Theorem 4 is directly implied by the Lemma, which asserts that under approval voting, if any candidate takes the position 50, then, in any voting equilibrium, all likely winners must be positioned at 50. Consequently, if no candidate were positioned at 50, then any candidate could make himself the unique likely winner by moving to 50. Therefore a positional equilibrium must have at least one candidate at 50, and it then follows from the Lemma that all likely winners will be at 50, as Theorem 4 asserts.

LEMMA. *Under approval voting, for any position vector x with at least one candidate positioned at 50, if $\mu \in E(x)$, then only candidates positioned at 50 can be likely winners in μ.*

Proof of Lemma. If all candidates are positioned at 50, the lemma is trivially true. Therefore, we assume that there is at least one candidate located at 50, and another is located elsewhere. Note that

$$G(p, v, u(\theta)) = \sum_i v_i \sum_{j \neq i} p_{ij}(u_i(\theta) - u_j(\theta))$$
$$= \sum_i v_i \sum_{j \neq i} p_{ij}((x_j)^2 - (x_i)^2 + 2(x_i - x_j)\theta).$$

A voter with ideal point θ will (at equilibrium) give an approval vote to any candidate i for whom the prospective rating

$$\sum_{j \neq i} p_{ij}(x_j^2 - x_i^2 + 2(x_i - x_j)\theta)$$

is positive and will not give an approval vote to any candidate for whom the prospective rating is negative. Assume that all of the perceived pivot probabilities are positive. Then the prospective rating of candidate i will be positive if $x_i = \theta$. Since the prospective rating is linear in θ, the set of all voters who cast an approval vote for candidate i must therefore contain either all voters with ideal points less than or equal to x_i or all voters with ideal points greater than or equal to x_i (depending on the sign of

$$\sum_{i \neq j} p_{ij}(x_i - x_j)).$$

In either case, a candidate located at 50 must have a predicted score (i.e., a per capita vote total) of at least 51/101.

Now take $\epsilon > 0$ and let p be a vector of positive pivot probabilities satisfying the ordering condition for ϵ. Let h be a candidate located at 50, and, of all candidates not positioned at 50, let i be the candidate positioned furthest from 50 with $S_i(\mu) \geq S_h(\mu)$. (If no such candidate exists, then we are done.) Decompose the prospective rating of candidate i for a voter with ideal point 50 into three terms:

$$\sum_{j \neq i} p_{ij}(u_i(50) - u_j(50)) = \sum_{\substack{S_j(\mu) \geq S_h(\mu) \\ j \neq h}} p_{ij}(u_i(50) - u_j(50))$$
$$+ p_{ih}(u_i(50) - u_h(50)) + \sum_{S_j(\mu) < S_h(\mu)} p_{ij}(u_i(50) - u_j(50)).$$

The first term is nonpositive, because of the extreme position of candidate i among all candidates with scores as high as that of candidate h. For any

$$\epsilon < (u_h(50) - u_i(50))/(k \max_{j \neq i} |u_i(50) - u_j(50)|),$$

the sum of the latter two terms is negative, because $p_{ij}/p_{ih} \leq \epsilon$ for all candidates j for whom $S_j(\mu) < S_h(\mu)$. Thus, for all sufficiently small ϵ, voters with ideal point 50 will not vote for candidate i. It follows that the set of ideal points of voters who vote for candidate i must be a subset of either {0, . . . , 49} or {51, . . . , 100}. In either case, candidate i's score cannot be more than 50/101. Hence, there can be no candidate positioned away from 50 with a score as high as that of candidate h; that is, all likely winners must be positioned at 50.[10] Q.E.D.

There is a large literature on candidate-positioning games similar to this game (see Shepsle 1991). Most of this literature considers only the plurality rule and assumes that voters vote "sincerely" ("honestly," "nonstrategically") for their most preferred candidate.[11] Feddersen, Sened, and Wright (1990) study plurality rule with strategic voters (and with entry costs for candidates), but they only consider pure-strategy equilibria of games with a fixed finite set of candidates and voters. Their result (that at equilibrium, all candidates who enter the election take the median position and receive equal vote totals) would not hold if they expanded the set of equilibria to include randomized equilibria, in which candidates have some uncertainty about the voters' behavior. Palfrey 1989 also considers plurality rule with strategic voters, and remarks that the multiplicity of voting equilibria makes the candidate-positioning game indeterminate, as our Theorem 3 asserts. However, he does not attempt to analyze the different implications of strategic voting under approval voting. Cox (1987, 1990) bases general comparisons of candidate positioning under different electoral systems on the assumption that voters vote sincerely. Cox (1985) obtains results for approval voting similar to our Theorem 4 but under somewhat different assumptions.

Advocates of a two-party system sometimes argue in favor of the plurality rule over alternative voting systems, claiming that plurality rule discourages the entry of third parties. Our results, restated in a two-candidates-plus-potential-entrant setting, support this claim but cast it in a rather unfavorable light. Under plurality rule, an election between two extremist candidates (emerging as the nominees of major parties after a season of primaries and conventions) can lead to the election of one of them, even if a more moderate candidate (who would defeat either of the others in a head-to-head race) enters the fray. Under approval voting, unless one of the major-party representatives takes a centrist position, an independent entrant can take that position and expect to win for certain. This does not mean that the use of approval voting would necessarily lead to disintegration of the two-party system. Rather, its use might discourage the major parties from nominating extremists in the first place.

MULTIPLE EQUILIBRIA AND THE FOCAL POINT EFFECT

The candidate-positioning game provides a striking contrast between plurality rule and approval voting. Under plurality rule we find that a multitude of voting equilibria supports a plethora of positional equilibria and that almost any policy outcome can occur in equilibrium. Under approval voting, the unique equilibrium policy outcome is at the median voter's ideal point. In our example, voters' preferences impose no constraint on the equilibrium policy outcome under plurality rule, whereas voters' preferences fully determine the equilibrium policy outcome under approval voting.

The multiplicity of voting equilibria for multicandidate elections under plurality rule is quite general. Let i and j be any two candidates such that a majority of voters prefers candidate i to candidate j and no voters are indifferent as between i and j. Under plurality rule, there will always be a voting equilibrium in which everyone votes either for i or for j and candidate i is the unique likely winner. Such an equilibrium is supported by the perception that the race between candidates i and j is the only serious race (i.e., $q_{ij} = 1$) and that therefore a vote for any other candidate would be wasted. Thus, any candidate i who is not a Condorcet loser can be the unique likely winner in a voting equilibrium under the plurality rule. (A candidate is a *Condorcet loser* if a majority would vote against him in a two-candidate race against any one of the other candidates.) Furthermore, in Example 1 we found an equilibrium under the plurality rule in which candidate 3 was the unique likely winner, even though he was a Condorcet loser. Thus, in any electoral situation, the set of candidates who can be unique likely winners at equilibrium under the plurality rule includes everyone who is not a Condorcet loser and may include some Condorcet losers, as well.

What is the political significance of the multiplicity of equilibria? The existence of a large set of equilibria in an electoral situation provides great political influence to political leaders and other individuals who have access to mass media; that is, the existence of multiple equilibria does not merely mean that outcomes are difficult to predict theoretically. The multiplicity of equilibria may also have substantive implications for the distribution of power in society.

Schelling (1960) argues that in games with multiple Nash equilibria, anything that tends to focus the players' attention on any one equilibrium may lead each player to expect the others to act in accordance with that equilibrium. With such expectations, each player does best for himself by also acting in accordance with the equilibrium; thus, the expectations are fulfilled. An equilibrium realized because of a focusing of attention is said to be *focal*.

In an electoral setting, if the voters come to believe some given prediction about the aggregate distribution of votes in an election, then their individual voting optimization decisions can confirm this prediction (consistent with ordered perceptions of the relative likelihood of various, close two-candidate races) if and only if the prediction corresponds to a voting equilibrium. If there are multiple voting equilibria, then there is more than one prediction about the outcome of the election that could be fulfilled by rational voter behavior if believed by the electorate. An individual who could focus the voters' expectations would then be in a position to pick any voting equilibrium and make a self-fulfilling prophecy.

Consider, again, our treatment of Example 1 under the plurality rule. The majority block of A- and B-type voters can avoid the worst possible outcome for themselves (the election of candidate 3) only by coordinating their focus on one of the two equilibria in which they all vote for the same candidate. Depending on the culture and traditions of the society in which the election takes place, almost any kind of public event could serve as a focal factor. For example, the equilibrium in which candidate 1 is the only likely winner might become focal if candidate 1 receives the endorsement of an important leader or organization, or if he wins a straw poll in a small (even unrepresentative) preelection caucus that is widely reported in the news media, or if he bears a well-known family name.

A *focal arbiter* is an individual whose opinions and statements about the candidates command wide attention, giving him influence upon the outcome of the election by making focal an equilibrium in which a candidate he supports has a significant chance of winning (or one he condemns does not). Such focal arbiters include party leaders; voters who participate in early preelection polls (e.g., the voters in Iowa and New Hampshire in American presidential elections); and others with access to the mass media (including, of course, media personalities themselves, who select their own interpretations of which candidate is the "true" winner in early primary elections). To become a focal arbiter, an individual does not need to have particularly good judgment in evaluating candidates or even to be perceived as having good judgment. It is only necessary that he be able to get his views prominently reported to the public. The power of focal arbiters is greatest in situations where there is a multitude of voting equilibria.

When analyzing the effects of various campaign events and activities on election outcomes, it is useful to distinguish between *focal manipulation* and *candidate presentation*. An event or activity serves the candidate presentation function when it affects voters' preferences over candidates by conveying information to the voters about the candidates and their positions on the issues. Obviously, the outcome of an election can be affected substantially by publicity that changes the voters' understanding of the candidates' positions and qualifications. An endorsement from a widely respected public figure who knows a candidate may be considered as candidate presentation if the endorsement helps to convince voters that this candidate has the ability to do a good job in office.

McKelvey and Ordeshook (1985) have studied a model in which preelection polls serve an implicit candidate presentation function by letting poorly informed voters make inferences about candidates' positions from the preferences of well-informed voters. In contrast, pure focal manipulation includes any activity whose effect is to influence the voters' beliefs about who will win the election and thus to direct voters' attention toward particular equilibria, without altering the voters' underlying utility vectors.

The existence of many voting equilibria in an electoral setting forces political campaigners not only to engage in effective candidate presentation but also to obtain the support of focal arbiters and engage in various forms of direct focal manipulation. Such support and manipulation would be much less important if there were only one equilibrium that voters could rationally expect.

The great multiplicity of equilibria under plurality rule means that plurality rule is well designed to serve the interests of focal arbiters. The fact that anyone who is not a Condorcet loser can be elected with the help of the appropriate focal manipulations implies that the set of possible equilibrium outcomes under plurality rule may have very little dependence on the voters' preferences. Consequently, focal manipulation may be much more important to a successful campaign than candidate presentation. Our results suggest that the use of other electoral systems (such as approval voting or Borda rule voting) may decrease the influence of focal arbiters as a class by decreasing the range of voting equilibria.

Notes

1. For present purposes, we restrict our attention to voting systems representable as "scoring rules." See Smith 1973 and Young 1975 for axiomatic perspectives on this restriction.

2. The vector (0, 0, 0) represents an abstention and is included for purposes of completeness. Since our analysis assumes costless voting, voters never prefer to abstain.

3. In assuming that a voter's payoff depends only on which candidate wins the election, we implicitly exclude the possibility that his utility payoff also depends on the vote totals of the various candidates (their "mandates") or on the ballot that he submits. Only the identity of the winner matters to the voters.

4. We assume that T is finite only for purposes of notational simplicity. Voters of the same type should *not* be viewed as forming a monolithic bloc: they may cast different ballots.

5. The voter population can be viewed either as fixed, with f(u) representing the specific fraction of the population with utility type u, or as random, consisting of a large number of independent random draws of voters according to the probability distribution f(·). Our results stand up to either interpretation.

6. To show that $Q^{\epsilon}(\mu)$ is nonempty for every election result μ, let $p_{ij} = \epsilon^{n}/k^{2}$, where n is the product of the number of candidates with predicted scores no less than candidate i's and the number of candidates with scores no less than j's.

7. Forsythe et al. (1990) present the results of a series of three-candidate voting experiments based on Example 1. In elections preceded by polls or prior elections, outcomes accord well with our equilibrium analysis. In particular, the vote totals of the three candidates are typically much closer to one another under approval voting and the Borda rule than under the plurality rule; yet the "minority" candidate (candidate 3 in our example) wins far less frequently than either of the other two candidates.

8. The voters' actions depend on the perceived probabilities of ties between specific pairs of candidates. A candidate's choice of a position, however, should depend on his perception of the probability that he will win the election if located at that position. Our definition of a positional equilibrium is complicated by the fact that we cannot derive any simple relationship between the pivot probabilities and the probability of any particular candidate's winning. In an earlier version, we used the simple assumption that the pivot probability for any pair of candidates was perceived to be proportional to the product of the candidates' probabilities of winning the election (as estimated from preelection polls), but we could not derive this relationship formally from any sensible stochastic model.

9. The placement of the two "serious" contenders at opposite ends of the political spectrum is not essential to the proof of Theorem 3. For any position $z \leq 50$, there is also (under the plurality rule) a positional equilibrium (x, μ) in which $(x_1, x_2, x_3) = (z, z/2, 50)$. In the election result μ, only candidates 1 and 2 have positive predicted scores, and candidate 1 is the sole likely winner. At this positional equilibrium, any change in position by candidate 2 can lead to the election of candidate 3; thus, candidate 2 not only might still lose but might even see adopted a position further from his original one.

10. Theorem 4 holds for any number of candidates $k \geq 2$, as the proof of the Lemma demonstrates. In the three-candidate case, the result would still hold if we only required that equilibria be justifiable by perceptions that assign a much greater likelihood to a tie involving the predicted top two vote getters than to a tie between any other pair of candidates. With four (or more) candidates, the full force of the ordering condition is needed. Consider a four-candidate race involving candidates located at $(x_1, x_2, x_3, x_4) = (10, 40, 40, 50)$. This situation has an "unordered" voting equilibrium in which voters with ideal points between 0 and 25 vote only for candidate 1; voters between 25 and 30 cast approval votes only for candidates 2 and 3; and voters above 30 vote for candidates 2, 3, and 4. The two candidates located at 40 are the only likely winners, and candidate 1 has the lowest predicted score. This election result is justified by perceptions p for which p_{23} is much larger than $p_{12} = p_{13}$, with all other pivot probabilities much smaller. However, the ordering condition would require that p_{24} and p_{34} be much larger than p_{12} and p_{13}. (This example clarifies the concerns expressed in Cox 1985, n. 4, concerning his inability to rule out the existence of "noncentrist" equilibria using only arguments based on the elimination of dominated strategies. Cox's feeling that such equilibria are rare is confirmed, in our framework, by the impossibility of justifying such election results with perceptions that satisfy the ordering condition for all positive ϵ.)

11. One attempt to generalize the notion of "sincere voting" to general scoring rules has been to require that a voter's ballot assign higher scores to more preferred candidates (i.e., if $u_i > u_j$, then $v_i \geq v_j$). This generalization is not fully determinate for a system such as approval voting. We offer here a stronger generalization. A *sincere ballot* is one that maximizes a voter's expected gain, given the perception that all ij–pivot probabilities are equal. With equal pivot probabilities, the expected-gain function G(v, p, u) is proportional to

$$\sum_{i \in K} v_i(u_i - \bar{u}),$$

where $\bar{u}$ represents the mean of all candidate utilities; that is

$$\bar{u} = \sum_{i \in K} u_i/k.$$

A model based on sincere voting would replace our ordering condition with the simpler assumption of equal perceived pivot probabilities. One of us elsewhere presents a comparison of approval voting with other voting systems, based on this sincerity assumption (Weber 1977).

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Roger B. Myerson and Robert J. Weber are Professors of Managerial Economics and Decision Sciences, Northwestern University, Evanston, IL 60208.

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GUNS, BUTTER, AND ANARCHY

ROBERT POWELL *University of California, Berkeley*

A state in the international system implicit in realism must allocate its limited resources between satisfying its intrinsically valued ends and the means of military power. I formalize this guns-versus-butter problem in a simple infinite-horizon model in which two states must continually decide how to allocate their resources and whether to attack the other state. The analysis establishes sufficient conditions to ensure the existence of an equilibrium in which neither state attacks; shows that there is a strictly Pareto-dominant pair of peaceful equilibrium payoffs; characterizes the unique, peaceful Markov perfect equilibrium that yields them; and describes the comparative statics of the equilibrium allocations. More broadly, the analysis also suggests that the notion of anarchy has little if any substantive significance distinctively related to international politics and that the problem of absolute and relative gains is superfluous.

In its starkest form, the international system implicit in realism is one in which each state must allocate its limited resources between satisfying its intrinsically valued, domestic ends—whether those ends be to promote the economic and social well-being of the citizenry or to suppress domestic opposition—and the means of military power. International politics is "a recurring struggle for wealth and power among independent actors in a state of anarchy" (Gilpin 1981, 7). I shall formalize this classic guns-versus-butter problem in a simple infinite-horizon model in which each of two states must continually decide both how to allocate its resources and whether to attack the other state or not. The analysis of this game establishes conditions sufficient to ensure the existence of a peaceful equilibrium, that is, an equilibrium in which neither state attacks. It also shows that there is a strictly Pareto-dominant pair of peaceful equilibrium payoffs and characterizes the unique, peaceful Markov perfect equilibrium that yields these payoffs.

The present model differs in several ways from other formal models that have been used to study the trade-off between internal ends and military means. In many arms race models, the states only decide on armament levels. The decision whether or not to go to war and the link between the states' relative military capabilities and the probability of prevailing in the event of war are not modeled explicitly (Brams, Davis, and Straffin 1979; Brams and Kilgour 1988, 16–37; Downs and Rocke 1990, 81; Gillespie, Zinnes, and Tahim 1977; Gillespie et al. 1977; Simaan and Cruz 1975). Insofar as no state in these models can actually use its arms against another state, there really is no reason for any state to acquire arms. Models that do explicitly incorporate the decision to attack have not generally been analyzed game-theoretically (Intriligator and Brito 1977, 1984; McGuire 1965). In contrast to those models, Brito and Intriligator (1985) study a well-defined game that incorporates this decision, as well as the link between relative military capabilities and the probable outcomes of fighting. However, their analysis centers on Nash equilibria that are not perfect. Consequently, a state in their model may be deterred by threats that it knows would not be in the threatener's self-interest to carry out if challenged. The present model explicitly represents the decision of whether or not to attack and the relation between the states' military capabilities and the probability of prevailing in a well-defined game analyzed in terms of its perfect equilibria.

This analysis contributes to international relations theory in three ways. First, many of the most influential contemporary approaches to international relations theory draw on more or less explicit analogies between the international system and oligopolistic markets. Waltz, for example, explains his notion of political structure specifically in terms of an analogy with market structures and oligopoly (1979, 89–94). For Gilpin, an "understanding of how structure constrains and influences the foreign-policy behavior of states is provided by the theory of oligopolistic competition" (1981, 87–88). And, Keohane's "modified structural research program" would, like realism, "be based on microeconomic theory, particularly oligopoly theory" (1986, 193). But with few exceptions (Brito and Intriligator 1985; Kim and Morrow 1991; Niou and Ordeshook 1986, 1987, 1990, 1991; Niou, Ordeshook, and Rose 1989; Wagner 1986), the formal economic models of oligopoly lack formal counterparts in international relations theory. The model developed here helps to bridge this gap.

The second contribution is conceptual clarification. Much recent work in international relations theory has focused on the significance of the shadow of the future in shaping states' behavior (Axelrod 1984; Axelrod and Keohane 1986; Keohane 1984; Lipson 1984; Oye 1986); on the effects of state's willingness to take risks and the closely related notion of resolve on the likelihood of war in general and the dynamics of crisis bargaining in particular (Bueno de Mesquita 1981, 1985; Jervis 1976, 1984, 1989; Morrow 1986, 1987; Powell 1987, 1988, 1990; Schelling 1966; Snyder and Diesing 1977); and on the relations among the offense–defense balance, the intensity of the security dilemma, and international stability (Herz 1950; Hopf 1991; Jervis 1978, 1988; Posen 1984; Quester 1977; Shimshoni 1990; Snyder 1984a, 1984b; Van Evera 1984a, 1984b; Waltz 1979). The model incorporates the shadow of the future, the states' willingness to run risks, the offense–defense balance, and the security dilemma in a single formulation. Bringing these concepts together in this way permits a unified examination that helps clarify these concepts individually and illuminates the relations among them.

In the model, increases in a state's expected payoff to fighting and its willingness to run risks, as well as shifts in the offense–defense balance toward the offense, lead to higher equilibrium military allocations. Unlike other analyses based on the repeated prisoner's dilemma (Axelrod 1984; Axelrod and Keohane 1986; Lipson 1984; Oye 1986), the shadow of the future does not promote greater welfare. A longer shadow of the future, that is, a higher discount factor, leads to greater military allocations and lower payoffs.

Finally, the model casts some doubt on the significance of several prominent concepts in international relations theory. The concept of anarchy and the related problem of absolute and relative gains have been central to much of international relations theory and the focus of much recent study (Gowa 1986; Grieco 1988a, 1988b, 1990; Hopf 1991; Jervis 1988; Keohane 1984; Mastanduno 1991; Mearsheimer 1990; Milner 1991; Oye 1986; Powell 1991; Snidal 1990; Waltz 1979). In what follows, the notion of anarchy has little if any substantive significance that is distinctively related to international politics and the problem of absolute and relative gains becomes superfluous.

I shall provide a less technical description of the model and summary of some of the results. I then give the core of the formal analysis, first formalizing the model in a game, second, characterizing the game's equilibria. Readers who are less interested in the technical analysis may pass over this part. Finally, I shall discuss some broader issues in international relations theory.

THE INTERNATIONAL DILEMMA

The basic idea motivating the model is that the international system confronts states with a dilemma. Each state has to decide how to allocate its limited resources between domestic, internal ends, and a military sector that acquires instrumental value because of the strategic environment created by the international system. Loosely, each state must decide how to divide its resources between guns and butter. In making this decision, each state is assumed to derive *direct* benefits solely from the present and future resources it devotes to satisfying its internal ends—how much butter it consumes today and in the future. Allocations to the military bring no direct benefits. But military allocations bring important indirect benefits because of the strategic setting defined by the international system. The more a state devotes to its military sector relative to other states, the stronger it will be and the easier it will be for that state both to defend its resources and, if it wishes, to conquer additional resources that may subsequently be devoted to meeting its internal ends. Conversely, the more a state allocates to satisfying its internal ends today and, consequently, the less it can allocate to the military, the weaker it will be tomorrow. Should another state then decide to attack, this military weakness means a higher probability of defeat and, in the event of defeat, fewer future resources to devote to future internal ends. Thus, the specter of war gives rise to a trade-off between current and expected future domestic allocations.[1]

The trade-off between resource allocations valued because they satisfy internal wants and those valued instrumentally because of the strategic setting of the international system may be described variously as a choice between satisfying domestic ends and military spending, consumption and defense, butter and guns, or wealth and power. But however described, the fundamental trade-off is the same and seems to define, if sometimes only implicitly, the very essence of the dilemma confronting the states in many approaches to international relations theory. Waltz, most explicitly, assumes that "power is a means and not an end" (1979, 126); that is, power is instrumentally, not intrinsically, valuable as a possible means to intrinsically valued ends. (For similar examples, see Gilpin 1981, 6, 18–25; Keohane 1984, 18–24; Wolfers 1962, 72, 89–102; and possibly Morgenthau 1966, 25–35).[2]

The model is intended to capture the trade-off between satisfying internal ends and military expenditures that are instrumentally valuable because of the strategic environment defined by the international system in a simple formal setting. The game lasts forever, that is, for infinitely many periods. At the beginning of the first period, one state, say S_1, must decide how to allocate a limited amount of resources, r_1, between its domestic ends and its military sector and whether or not to attack the other state. If S_1 decides not to attack, then the other state, S_2, begins the second period by deciding how to divide its resources and whether or not to attack. If S_2 does not attack, then S_1 starts the third period by deciding how to allocate its resources of r_1 and whether or not to attack. These alternating allocation decisions continue as long as neither state attacks.

If one of the states decides to attack at some time, then the ensuing war can end in one of only two ways in this simple formulation. Either S_1 will prevail by conquering S_2 and eliminating it as a military power, or S_2 will prevail by eliminating S_1. The probability of victory is assumed to be a function of the relative sizes of the states' military allocations existing when the war occurs. In particular, the more a state has allocated to its military sector relative to the other state, the more likely it is to prevail in the event of war. War, in effect, transforms the bipolar system into a monopolar system because defeat eliminates the loser. In the strategic setting created by this new "international" system, military spending no longer has any instrumental value. There is no other state to threaten the victor and nothing more for it to conquer. Consequently, the victor can devote all of the resources it had before the war, as well as any additional conquered resources to its domestic ends. The conquered state, having been eliminated as a military power, is prevented by the victor from rearming. Thus, any resources that the defeated state may have after the war go toward its domestic ends.

It may help make the model more concrete to think of r_1 and r_2 as the capital stocks of S_1 and S_2, respectively. At the beginning of the first period, S_1 must decide what fraction of its capital stock, r_1, to dedicate to producing consumption goods and to military goods and whether or not to attack S_2. If S_1 does not attack, S_2 must decide how to allocate its productive resources and whether or not to attack S_1 in the second period. Resources allocations are sticky in the sense that once S_1 allocates its resources in the first period, they cannot be immediately reallocated in the second. Rather, S_1's allocation in the first period also fixes its resource allocation in the second period, as well. But if S_2 does not attack in the second period, the third period starts with S_1's having to decide how to allocate its resources and whether or not to attack. These alternating allocation decisions continue as long as neither state attacks.

If one of the states attacks, then the ensuing war leads to a redistribution of the capital stock that is not destroyed in the fighting. The victor, having eliminated the other state as a military power, now can dedicate its entire capital stock to producing consumption goods. The vanquished is constrained to remain disarmed and devote whatever resources may be left to it to consumption.

As long as neither state attacks, the states' capital stocks remain constant. There is implicitly no growth, no investment, and no depreciation in the model. The states' production possibilities remain constant as long as peace prevails. Only war can destroy some of the capital stock. This assumption greatly simplifies the formal analysis but unfortunately leaves the present formulation silent on the relation between current military spending and long-run economic growth that could allow a state to allocate more in the future to both consumption and defense.

The goal of each state in the model is to maximize the absolute level of resources allocated to satisfying its internal ends; that is, each state's utility is assumed to be solely a function of the current and future resources it dedicates to domestic ends. Put another way, each state's utility is a function only of its absolute gain. As shall be specified more formally later, each state's utility function also reflects its willingness to run risks and its concern about the future.

What to assume about state preferences has been a contentious issue in international relations theory. Two comments are in order here. First, neoliberal institutionalism holds that states focus primarily on their absolute gains and are indifferent to the gains of others, while neorealism or structural realism asserts that states are largely concerned with relative gains. Indeed, "relative gains are more important than absolute gains" in structural realism (Waltz 1959, 198). This difference between the relative importance of absolute and relative gains has generally been traced to different assumptions about state preferences and formalized by assuming that a state's utility is solely a function of its absolute gain in neoliberal institutionalism and of some relative measure like power in structural realism.[3]

The present model assumes that the states are trying to maximize their absolute gains in the form of their domestic allocations. This assumption, while in accord with neoliberal institutionalism, may appear to make the model incompatible with, and irrelevant to, structural realism. But as will be seen, the model is very much in keeping with structural realism. What is more, the fact that a model in which states are explicitly trying to maximize their absolute gains is in keeping with structural realism suggests that casting the differences between structural realism and neoliberal institutionalism in terms of state preferences is misguided.

Second, this model, like most rational choice models, takes the actors' preferences as given and assumes them to remain constant. States are taken to be rational unitary actors with well-behaved transitive preferences. This is a simplifying theoretical assumption common to many structural approaches to international relations theory that hope to abstract away from the aggregation problems inherent in collective actors.[4] However, recent work in international relations theory has raised the problem of preference formation and questioned the assumption that preferences remain constant (Jervis 1988, 322–29; Keohane 1984, 116–20; Keohane 1986, 191–92; Keohane and Nye 1987, 742). This concern is especially important for theories, whether formal or not, that attempt to encompass long periods of time during which either the domestic coalitions and institutions that presumably underlie a state's preferences or the very nature of what it means to be a state may change. Such changes certainly occur, and Arrow's theorem shows that treating collective actors as unitary actors may be problematic.[5] The presumption here is that a model based on simplifying assumptions about state preferences may provide interesting insights and, perhaps, the foundation for future work that can relax some of the most demanding assumptions. Evaluating this presumption means examining the model and what can be derived from it.

As noted, the use of force is modeled very simply. There is no possibility of an inadvertent war; and when there is a war, it can end in only two ways. Either S_1 eliminates S_2 as a military power, or S_2 eliminates S_1. The probability that a state will prevail depends on three factors: the amount of resources S_1 is devoting to the military when the war is fought, say m_1; the amount of resources S_2 is allocating to the military when the war occurs, say m_2; and a parameter ω that measures the balance between the offense and defense.

The offense and defense are perfectly balanced, so that there are no offensive or defensive advantages, when $\omega = 1$; that is, a state's probability of victory at any combination of military allocations (m_1, m_2) is the same whether that state attacks or is attacked. Thus, there is no difference between striking first or being struck first if $\omega = 1$. The offense dominates the defense when $\omega > 1$. With $\omega > 1$, the probability that a state will prevail if it attacks at a given combination of allocations (m_1, m_2) is greater than the probability that it will prevail if it is attacked at this combination of allocations. The defense dominates if $\omega < 1$. In this case, the probability that a state will prevail if it attacks at a particular combination is less than the probability that it will prevail if it is attacked at this combination. In this way, the model incorporates the offense–defense balance.

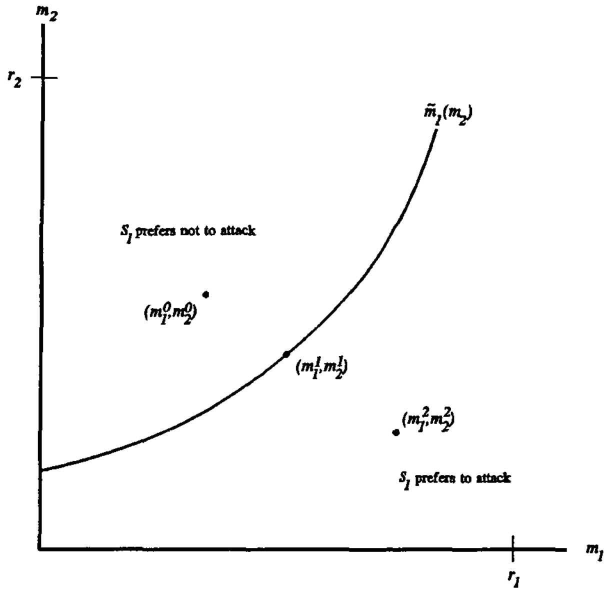
The model also reflects the notion of a security dilemma. To reduce the probability of defeat if attacked, a state must increase its defense allocation. But one consequence of this, whether intended or not, is to increase the probability that the other state will be defeated if attacked. Increasing one's own security adversely affects another's; this is the essence of the security dilemma.

Turning to a discussion of the game's equilibria, a state's strategy specifies how much it will allocate to the military and whether or not it will attack as a function of the other state's existing military allocation.[6] An equilibrium is a pair of strategies such that neither state has any incentive to deviate from its strategy regardless of the other state's military allocation.[7] An equilibrium describes how the two states will interact and how the game will unfold. In particular, the equilibrium strategies specify how much each state devotes to its internal ends and to its military in each period. This, in turn, makes it possible to calculate the state's equilibrium payoffs and to examine how these payoffs vary with changes in the state's willingness to run risks, in their concern about the future, and in the offense–defense balance and the intensity of the security dilemma.

At the most basic level, one would like to know if there are equilibria in which neither state ever attacks the other. Can the system be peaceful? To answer this question, it will be useful to define a peaceful equilibrium as an equilibrium in which neither state attacks and to consider two functions, $\tilde{m}_1(m_2)$ and $\tilde{m}_2(m_1)$. To define these two functions, suppose that each point in the (m_1, m_2) plane in Figure 1 represents a different combination of military allocations. At, for example, (m_1^0, m_2^0), S_1 is allocating m_1^0 to the military and $r_1 - m_1^0$ to its internal ends, and S_2 is devoting m_2^0 to the military and $r_2 - m_2^0$ to its domestic ends. The first of the two functions, $\tilde{m}_1(m_2)$, divides the plane into three regions. At allocations to the left of $\tilde{m}_1(m_2)$, say (m_1^0, m_2^0) in Figure 1, S_2 prefers remaining at (m_1^0, m_2^0) forever to reallocating and attacking S_2 at m_2^0; that is, if given a choice between having $r_1 - m_1^0$ to devote to its internal ends in every period and attacking S_2 at m_2^0, S_1 would choose to stay at (m_1^0, m_2^0). The reason for this choice is that at points to the left of $\tilde{m}_1(m_2)$, like (m_1^0, m_2^0), S_1's military allocation is relatively small compared to S_1's total resources and to S_2's military allocation. This has two effects. First, S_2's large military allocation means that S_1's probability of victory will be small. This makes the payoff to attacking small. Second, S_1's payoff to staying at (m_1^0, m_2^0) is relatively large because its military allocation is small and, consequently, its status quo payoff in each period, $r_1 - m_1^0$, is high. At allocations to the left of $\tilde{m}_1(m_2)$, the high payoff associated with the status quo outweighs the payoff to attacking; and S_1 prefers not to attack. At allocations along the curve $\tilde{m}_1(m_2)$, like (m_1^1, m_2^1) in Figure 1, S_1 is indifferent between remaining at that point forever and attacking. At allocations to the right of $\tilde{m}_1(m_2)$, like (m_1^2, m_2^2), S_1 prefers to attack.

FIGURE 1

S_1's Preferences for Attacking



The analogous function $\tilde{m}_2(m_1)$ divides the (m_1, m_2) plane into regions in which S_2 prefers not to attack, is indifferent, or prefers to attack. At points like (m_1^2, m_2^2) in Figure 2, which lie below $\tilde{m}_2(m_1)$, S_2's internal allocation is sufficiently high and its probability of victory is sufficiently low that it prefers remaining at (m_1^2, m_2^2) to attacking S_1 at m_1^2. At points along $\tilde{m}_2(m_1)$, S_2 is indifferent. At points like (m_1^1, m_2^1), which lie above $\tilde{m}_2(m_1)$, S_1's relatively small military allocation makes S_2's payoff to attacking high and S_2's large military allocation makes S_2's status quo payoff low. S_2 now prefers to attack.

It is important to emphasize that $\tilde{m}_1(m_2)$ and $\tilde{m}_2(m_1)$ are not reaction functions; that is, S_1's equilibrium strategy is not to react to m_2 by simply allocating $\tilde{m}_1(m_2)$ to the military. The functions $\tilde{m}_1$ and $\tilde{m}_2$ merely divide the (m_1, m_2) plane into various regions.

FIGURE 2

S_1's and S_2's Preferences for Attacking

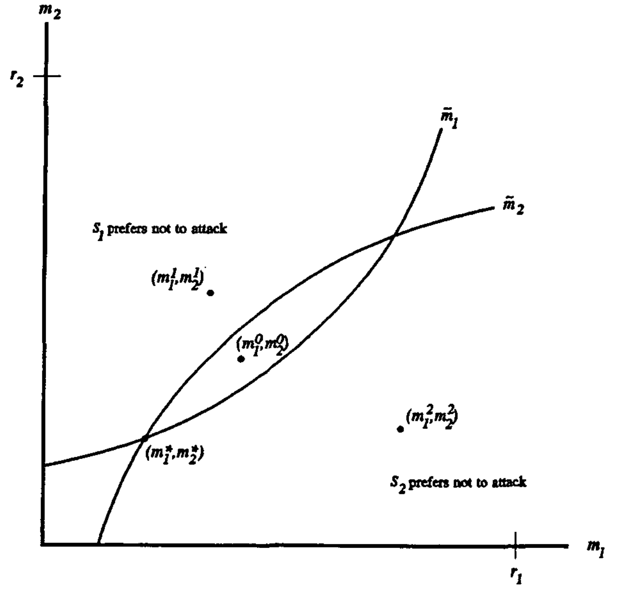
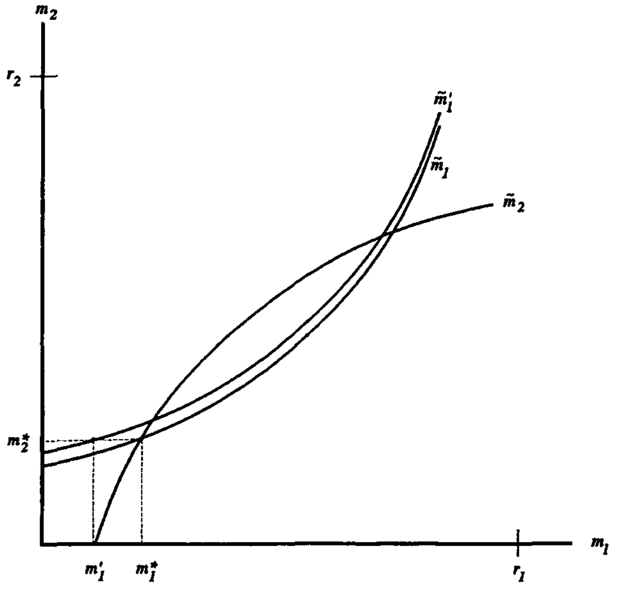


FIGURE 3

The Effects of Shifts in $\tilde{m}_1$ on Military Allocations



(Proposition 3 will characterize the states' equilibrium reaction functions.) Although not reaction functions, $\tilde{m}_1$ and $\tilde{m}_2$ make it easy to determine whether the system is stable in the sense of having a peaceful equilibrium. An allocation is stationary if in equilibrium, the states stay at that allocation forever; that is, (m_1^0, m_2^0) is stationary if S_1 always allocates m_1^0, and S_2 always devotes m_2^0, to the military. It follows immediately from this definition that if a peaceful stationary equilibrium exists, then it must lie in the lens formed by the intersection of $\tilde{m}_1$ and $\tilde{m}_2$. To see this, consider a point like (m_1^1, m_2^1) in Figure 2. This cannot be a stationary equilibrium outcome, because one of the states can improve its payoff by deviating from this allocation. In particular, (m_1^1, m_2^1) lies above $\tilde{m}_2$, so S_2 prefers deviating from this point by reallocating and attacking S_1 at m_1^1. Similarly, a point like (m_1^2, m_2^2), which lies to the right of $\tilde{m}_1$, cannot be a stationary equilibrium, because S_1 prefers to attack. Thus, any stationary peaceful equilibrium allocation must lie in the lens. Conversely, if these functions do not intersect, then no stationary peaceful equilibrium exists. Proposition 1, which will be developed more formally later, generalizes this result. It shows that if the two functions do not intersect, then no peaceful equilibrium, whether stationary or not, exists.

A necessary condition for the existence of a peaceful equilibrium allocation is, therefore, that $\tilde{m}_1$ and $\tilde{m}_2$ intersect. Sufficient conditions to ensure the existence of a peaceful equilibrium also exist. Once again, it is helpful to start with peaceful stationary equilibria.

Suppose that $\tilde{m}_1$ and $\tilde{m}_2$ intersect so that the necessary condition is satisfied. As just shown, any peaceful stationary equilibrium outcome must lie in this intersection. Compare the allocation (m_1^*, m_2^*) at the lower tip of the lens to any other point lying in the lens, say (m_1^0, m_2^0). Figure 2 suggests—and the formal analysis below shows—that (m_1^*, m_2^*) must lie to the lower-left of (m_1^0, m_2^0); that is, $m_1^* < m_1^0$ and $m_2^* < m_2^0$. Consequently, S_1 prefers (m_1^*, m_2^*) to (m_1^0, m_2^0), because its internal allocation in the former, $r_1 - m_1^*$, is greater that its internal allocation in the latter, $r_1 - m_1^0$. Similarly, S_2 prefers (m_1^*, m_2^*) to (m_1^0, m_2^0). Because both states prefer the former to the latter, (m_1^*, m_2^*) Pareto dominates (m_1^0, m_2^0). In sum, the payoffs associated with the allocations at the lower tip of the lens Pareto dominate any other peaceful stationary equilibrium payoffs. As before, this result can be generalized. Proposition 2 will show that these payoffs Pareto dominate any peaceful equilibrium payoffs, regardless of stationarity.

Proposition 2 will demonstrate that if the combination of allocations at the lower tip of the lens is, in fact, a peaceful equilibrium outcome, then each state will do better at this outcome than at any other peaceful equilibrium outcome. It does, however, remain to be shown that a pair of equilibrium strategies producing this Pareto dominant outcome actually exists. This is the task of proposition 3. It will characterize the unique peaceful equilibrium strategies that yield this outcome.

As I have shown, the model reflects the states' willingness to run risks, the shadow of the future, and the offense–defense balance. This and proposition 3's characterization of the game's equilibria make it possible to examine how changes in these elements affect the equilibrium allocations and the system's stability. Figure 3 aids the analysis. It shows how shifts in the functions $\tilde{m}_1$ and $\tilde{m}_2$ affect the equilibrium allocations. If, for example, $\tilde{m}_1$ shifts in to $\tilde{m}_1'$, then the lower tip of the lens shifts up and to the right. Both states' equilibrium military allocations increase. Similarly, anything that shifts $\tilde{m}_2$ down leads to larger military allocations, while anything that shifts $\tilde{m}_1$ in or $\tilde{m}_2$ up yields smaller military allocations.

In general, anything that raises the expected value of attacking for S_1 shifts $\tilde{m}_1$ in and produces higher military allocations, while anything that reduces the expected value of attacking for S_2 shifts $\tilde{m}_2$ down. Suppose, for example, that S_1 becomes more aggressive in the sense that it becomes risk-acceptant. This makes the gamble of attacking relatively more attractive and shifts $\tilde{m}_1$ in to, say, $\tilde{m}'_1$. To see this, consider any point along $\tilde{m}_1$, say (m_1^*, m_2^*) in Figure 3. By construction, S_1 is indifferent between being at this point forever and to the gamble of reallocating its resources and attacking S_2 at m_2^*. Once S_1 becomes more risk-acceptant, it strictly prefers the gamble of attacking to remaining at (m_1^*, m_2^*); that is, S_1 prefers attacking to devoting $r_1 - m_1^*$ to its internal ends. But at some point to the left of (m_1^*, m_2^*), say (m_1^1, m_2^*), S_1 is just indifferent to devoting $r_1 - m_1^1$ to its internal ends in every period and to reallocating and attacking. This, however, is what it means to be on $\tilde{m}_1$. Thus, an increase in S_1's willingness to run risks shifts $\tilde{m}_1$ in and leads to the higher military allocations at the lower tip of the lens formed by the intersection of $\tilde{m}'_1$ and $\tilde{m}_2$.

Loosely, an increase in S_1's willingness to run risks means that S_1 begins to prefer attacking to the status quo of (m_1^*, m_2^*). Accordingly, S_2 increases its military allocation to deter S_1 from attacking by reducing S_1's probability of victory. But the larger military allocation needed to deter S_1 leaves S_2 with a smaller internal allocation. Consequently, S_2 now prefers attacking, because its internal allocation is less than it was at (m_1^*, m_2^*) where S_2 was just indifferent to the status quo of (m_1^*, m_2^*) and attacking because (m_1^*, m_2^*) was on $\tilde{m}_2$. In these circumstances, S_1 must increase its military allocation to deter S_2 from attacking. The result of this strategic interaction is higher military allocations.

The Appendix analyzes the effects of changes in the model's parameters on the peaceful equilibrium allocations formally. Summarizing those effects here, the peaceful Pareto dominant military allocations rise if either state becomes more aggressive in the sense that it becomes more willing to run the risk of starting a war. The higher the payoff to fighting or the lower the cost to fighting, the higher the states' military allocations. Similarly, the longer the shadow of the future looms in the model, the more the states devote to the military, the less they allocate to their internal ends, and the lower their payoffs.

This last result contrasts sharply with the inferences drawn from other work in international relations theory that uses the repeated prisoner's dilemma to model the international system. Those analyses generally argue that "concern about the future helps to promote cooperation" and increases the states' payoffs in the long run (Axelrod and Keohane 1986, esp. 232; see also Axelrod 1984; Lipson 1984; Oye 1986); that is, the more a state cares about the future relative to the present, the more likely cooperation is and the higher a state's expected payoff.

An intuitive explanation for these conflicting results lies in the different ways that changes in the length of the shadow of the future (i.e., changes in the states' discount factors) affect the payoff to deviating in the repeated prisoner's dilemma and in the present model. In repeated games, including the repeated prisoner's dilemma, deviation brings an immediate gain and future costs when the other actors begin to punish the defector. Because it is immediate, the gain is unaffected by the length of the shadow of the future. This shadow does, however, affect the future costs that can be imposed on the defector. The more an actor cares about the future (i.e., the higher its discount factor), the more it values the future losses that can be imposed on it. In a repeated game, therefore, a longer shadow of the future facilitates cooperation by raising the future costs of defection, because it makes it more likely that the future costs of defecting will outweigh the immediate gains. In the present model, the pattern of costs and benefits is reversed; this reversal means that a longer shadow of the future leads to higher military allocations and lower payoffs. If a state deviates by attacking, it suffers an immediate loss as it arms for war and reduces its internal allocation. The incentive to attack—if there is one—is that the expected future internal allocations that attacking may bring will exceed those of the status quo allocation. Thus, the gain to deviating by attacking is not an immediate gain, but a future one. This implies the longer the shadow of the future, the higher the gain to deviating from the status quo. To trace the effects of this greater incentive to attack, consider an equilibrium allocation like (m_1^*, m_2^*) in Figure 3, where each state is indifferent between staying at this allocation forever and attacking. If one of the states, say S_1, becomes more concerned with the future, it will strictly prefer attacking to remaining at (m_1^*, m_2^*). Fearing attack, S_2 will increase its military allocation to deter S_1. But this larger allocation means less goes toward satisfying S_2's internal ends. Consequently, S_2 now prefers attacking S_1 at m_1^*, and S_1 must increase its military allocation to deter S_2. In this way, a longer shadow of the future leads to larger military allocations.

Finally, the model illuminates the relation between the offense–defense balance and stability. This relation has been the focus of much recent work, which generally argues that offense dominance and first-strike advantages are destabilizing (Jervis 1978, 1988; Quester 1977; Posen 1984; Snyder 1984a, 1984b; Van Evera 1984a, 1984b). The greater the offensive advantage, the more likely war is believed to be. The model qualifies this. A shift of the offense–defense balance in favor of the offense does not necessarily make war more likely. An offensive shift does lead to higher military allocations. In terms of Figure 3, an increase in ω shifts $\tilde{m}_1$ in and $\tilde{m}_2$ down which moves the lower tip of the lens up. But as long as $\tilde{m}_1$ and $\tilde{m}_2$ intersect, a peaceful equilibrium continues to exist; and the net effect of these changes is not an increase in the likelihood of war.[8] If, however, the shift is sufficiently large that $\tilde{m}_1$ and $\tilde{m}_2$ no longer intersect, then no peaceful equilibrium exists and the system becomes unstable.

THE MODEL

The model is an infinite-horizon game between two states, S_1 and S_2. At the beginning of the first period, S_1 must decide how to allocate its resources, r_1, between its internal ends and its military sector and whether or not to attack S_2. If S_1 does not attack, then S_2 starts the second period by deciding how to divide its current resources, r_2, between its internal ends and its military sector and whether to attack S_1. If S_2 does not attack, then the third period begins with S_1's having to decide again how to allocate r_1. If S_1 does not attack, then S_2 must again decide how to divide its replenished resources of r_2 between internal ends and the military and whether to attack. The game continues in this way, possibly forever, as long as neither state attacks.

If neither state ever attacks, then the states' alternating allocation decisions generate an infinite sequence or stream of internal allocations for each state. The utility that S_1 attaches to its sequence of internal allocations is

$$\sum_{t=0}^{\infty} \delta_1^t (r_1 - m_1(t))^{\rho_1}$$

where, for notational convenience, the first period is denoted by $t = 0$.[9] The parameter δ_1 is S_1's discount factor. The higher δ_1, the more a state values future internal allocations and the longer the shadow of the future. The parameter ρ_1 reflects the state's attitude toward risk. If $\rho_1 = 1$, then S_1 is risk-neutral; if $\rho_1 < 1$, S_1 is risk-averse; and if $\rho_1 > 1$, S_1 is risk-acceptant. S_2's utility function is defined analogously.[10]

To specify the states' payoffs if there is an attack, suppose that S_1 attacks S_2 at time t_0. Fighting is assumed to affect the states' payoffs in the periods following the war. Accordingly, the utility that S_1 derives from the stream of internal allocations preceding the war is

$$\sum_{t=0}^{t_0} \delta_1^t (r_1 - m_1(t))^{\rho_1}.$$

S_1's attack can lead to one of only two outcomes in this simple stylization of war. Either S_1 will conquer S_2 and eliminate it as a military power, or S_2 will eliminate S_1 as a military power. The probability that S_1 will prevail is taken to be a function of the states' military allocations existing when the attack occurs, $(m_1(t_0), m_2(t_0))$ and a parameter ω representing the state of the offense-defense balance. This function will be discussed more fully later; for now, simply denote it by $\pi(m_1(t_0), m_2(t_0), \omega)$.

If S_1 prevails, then it is assumed to control v_1 resources in each period following the war. The net change in resources, $v_1 - r_1$, implicitly represents two factors: (1) any additional resources that S_1 acquires from S_2 and (2) the resources destroyed in the war. If $v_1 - r_1 > 0$, the former is larger than the latter, and prevailing brings S_1 a net gain in the resources it controls. If $v_1 - r_1 < 0$, prevailing brings S_1 a net loss.

S_1's defeat of S_2 eliminates S_2 as a military power for the rest of the game. The effect of this on S_1's payoff is that, having destroyed S_2 as a military power, S_1 may now devote all of its resources to its internal ends. Consequently, the utility S_1 receives from devoting v_1 to its internal ends starting in the period immediately following the war is

$$\sum_{t=t_0+1}^{\infty} \delta_1^t v_1^{\rho_1}.$$

The idea underlying this specification of S_1's payoff is that one of the benefits of conquering S_2 is that S_1 can prevent S_2 from rearming and can therefore reduce its own military allocations. That the victor can keep the vanquished from rearming forever is clearly a strong assumption, as the failed efforts to prevent German rearmament after World War I suggest. The primary reason for making this assumption is analytic tractability. Once there has been a war in the model, there is no further strategic interaction, which simplifies the formal analysis.

To specify S_1's payoff if it is defeated, let d_1 be the resources that remain to it after its defeat. Then S_1's utility will be taken to be

$$\sum_{t=t_0+1}^{\infty} \delta_1^t d_1^{\rho_1},$$

where the summation represents the utility S_1 derives from having d_1 resources that it is constrained by S_2 to allocate solely to its internal ends.[11]

Given these considerations, S_1's expected utility to attacking at t_0 is

$$F_1(t_0) = \sum_{t=0}^{t_0} \delta_1^t (r_1 - m_1)^{\rho_1} + \pi(m_1(t_0), m_2(t_0), \omega) \sum_{t_0+1}^{\infty} \delta_1^t v_1^{\rho_1}$$
$$+ (1 - \pi(m_1(t_0), m_2(t_0), \omega)) \left(\sum_{t_0+1}^{\infty} \delta_1^t d_1^{\rho_1} \right).$$

The first term in the expression is the utility that S_1 derives from its internal allocations preceding the war. The second is the probability of victory *times* the payoff to prevailing. The third is the probability of defeat *times* the payoff to that.

To describe the probability that an attacker will prevail more fully, let m^a be the attacker's allocation and m^d be the defender's allocation when the attack occurs. Then the probability that the attacker will prevail, $\pi(m^a, m^d, \omega)$ will be assumed to be increasing in the attacker's military allocation and decreasing in the defender's allocation. Moreover, π will be taken to be continuous except possibly at $(m^a, m^d) = (0, 0)$.[12]

To specify how π depends on the offense–defense balance parameterized by ω, consider the example $\pi(m^a, m^d, \omega) = \omega m^a/(\omega m^a + m^d)$. Now, if $\omega = 1$, then the probability that S_1 will prevail becomes $m_1/(m_1 + m_2)$, which is just the ratio of S_1's military allocation to the sum of both states' military allocations. To see how ω modifies this simple ratio to reflect the balance between offense and defense, note that the probability that S_1 will prevail if S_2 strikes first when $\omega = 1$ is $1 - m_2/(m_1 + m_2) = m_1/(m_1 + m_2)$. Thus, when $\omega = 1$, the probability that S_1 will prevail is the same whether S_1 strikes first or is struck first. There are no first-strike advantages. More generally, the difference between the probability that S_1 will prevail if it strikes first or second is $\omega m_1/(\omega m_1 + m_2) - (1 - \omega m_2/(m_1 + \omega m_2))$. This expression is increasing in ω, positive if $\omega > 1$, and negative if $0 < \omega < 1$. Therefore, when $\omega > 1$, S_1 is more likely to prevail if it attacks than if it is attacked. The larger ω, moreover, the greater the advantage to striking first. Conversely, when $\omega < 1$, S_1 is more likely to prevail if it is attacked than if it attacks; and the smaller ω, the better it is to be on the defensive. Thus, the offense dominates if $\omega > 1$, and the defense dominates if $\omega < 1$ in the particular example in which $\pi(m_1, m_2, \omega) = \omega m_1/(\omega m_1 + m_2)$. Moving beyond this specific example, $\pi(x, y, \omega)$ will be assumed to be increasing in ω; and the difference between the probability of prevailing if striking first or second, $\pi(x, y, \omega) - \pi(y, x, \omega)$, will also be taken to be increasing in ω and to equal zero at $\omega = 1$.

In sum, the formalization reflects many areas of recent and continuing work in international relations theory, albeit in a very simple and highly stylized way. First and most importantly, the model reflects the trade-off between domestic ends and military means that acquire instrumental value through the strategic setting of the international system. This dilemma seems to lie at the heart of many conceptual approaches to international relations theory. More narrowly, the model explicitly incorporates the shadow of the future in the states' repeated interaction and their discount factors δ_1 and δ_2, the states' attitudes toward risk in ρ_1 and ρ_2, the offense–defense balance in ω, and the security dilemma.

Before turning to an analysis of this game's equilibria, it is important to emphasize that the model makes four very demanding simplifying assumptions. First, there are effectively no stocks in the model; there are only flows. This means in part that a state cannot stockpile armaments in one period for use in future periods. However much a state allocates to the military in any period is fully "consumed" in that period. Nor can a state "save" in order to be able to devote more to both its internal ends and military in the future.

Second, there are no constraints on the amount of resources a state can reallocate. In the model, a state can go from allocating essentially nothing to its military in one period to allocating all of its resources to the military in the next. The effect of this is that a state can optimally reallocate its resources and then attack before the other state can reallocate any of its resources. This potential for an extreme "breakout" would seem to bias the model toward greater instability. At the other extreme, one might assume that a state must decide whether or not to attack with the allocations that exist at the beginning of the period and then reallocate only if it does not attack. This would seem to bias a model based on this assumption toward greater stability. Between these two extremes, one might allow a state to reallocate only a fraction of its resources before attacking. While potentially very interesting, the latter two assumptions lead to models that are more difficult to analyze.

Third, assumptions about the possible outcomes that may result from the use of force are extremely important. They largely shape the state's strategic environment. The simple assumption made here is that a war can end in only two ways. One state will prevail, and the other will be eliminated. Better models would allow for a range of possible outcomes.[13]

Finally, there is complete information in the model. Each state, for example, knows the other state's payoffs. A richer model would allow for asymmetric information.[14]

THE EQUILIBRIA

I shall formally characterize the game's equilibria. In particular, I shall identify conditions necessary for the existence of a peaceful Nash equilibrium, that is, a Nash equilibrium in which the probability of attack in equilibrium is zero. I shall also show that there is a strictly Pareto dominant pair of peaceful equilibrium payoffs and describe the unique, peaceful Markov perfect equilibrium that yields these payoffs.

A Markov perfect equilibrium is a perfect equilibrium in which the actor's strategies satisfy a special Markov property that simplifies the equilibrium.[15] In a Markov perfect equilibrium, the actors' strategies or "reaction functions depend only on the payoff-relevant state of the system" (Maskin and Tirole 1988a, 553). In this model, the payoff-relevant state of the system is easy to describe. At the beginning of each period, one of the states, say S_1, must allocate its resources and decide whether or not to attack S_2. S_1's payoff is solely a function of S_2's existing military allocation, which S_2 determined in the previous period and of the fixed parameters of the model. What S_2 may have done two or more periods ago has no direct effect on S_1's utility. Thus, the payoff-relevant state of the system for S_1 in any period in which it must act is simply the military allocation that S_2 made in the previous period. Similarly, the payoff-relevant state of the system for S_2 in a period in which it must make a decision is S_1's current military allocation.

Given this simple description of the payoff-relevant state of the system, a Markov strategy for S_1 specifies S_1's military allocation and whether or not it will attack S_2 solely as a function of the possible ways that S_2 could have allocated its resources in the previous period. Similarly, a Markov strategy for S_2 defines S_2's military allocation and its decision of whether or not to attack solely as a function of S_1's possible military allocations.

The analysis centers on Markov perfect equilibria for several reasons. The first and most important is that these equilibria are subgame perfect. In a subgame perfect equilibrium of the model, neither state has any incentive to deviate from its equilibrium strategy at the beginning of any subgame. This contrasts with a Nash equilibrium, which merely requires that neither state have any incentive to deviate from its equilibrium strategy along the equilibrium path. The substantive effect of this technical requirement is that in a subgame perfect equilibrium, unlike a Nash equilibrium, a state cannot rely on threats that (although in the state's interest to make) would not be in its interest to carry out should the time come. Thus, threats in a subgame perfect equilibrium must be credible in the sense that carrying them out must in the threatener's own self-interest. Consequently, subgame perfect equilibria are more compatible than Nash equilibria with the definition of international anarchy as "a lack of a common government" that can enforce threats or promises that may have been in a state's interest to make but not to carry out (Axelrod and Keohane 1986, 226).[16] For these reasons, subgame perfection formalizes the condition of international anarchy in a natural way.[17]

Second, Markov perfect equilibria eliminate "bootstrapping." Without the Markov restriction, a state in a subgame perfect equilibrium that is not Markov might react to allocation decisions made two or more periods ago, which have no direct effect on its expected utility, only because that state expects the other state to react to these distant allocations. Markov perfect equilibria avoid bootstrapping, which is inherent in many subgame perfect equilibria, by requiring a state to react only to factors that directly affect its utility.[18]

Finally, Markov strategies provide a natural formalization for the notion of a reaction function that underlies much of the existing work on arms races;[19] that is, a Markov strategy specifies how a state will react to the other state as a stationary function of the state of the system. Accordigly, the solution to the present model (i.e. the states' Markov perfect equilibrium strategies) might be seen as corresponding to Richardson's (1960) arms race reaction functions.

To describe a Markov perfect equilibrium of the present game more formally, assume that neither state has yet attacked and consider S_1's allocation decision. A Markov strategy for S_1 must specify what S_1 would do in response to every possible payoff-relevant state of the system, that is, to every possible allocation that S_2 might have made in the previous period. A pure Markov strategy for S_1 is therefore a function from the set of S_2's possible allocations, $[0, r_2]$, into the set of S_1's possible actions, $[0, r_1] \times \{A, \sim A\}$, where the first component of any point in $[0, r_1] \times \{A, \sim A\}$, say $\{m_1, \sim A\}$ is S_1's military allocation and the second component reflects S_1's decision about attacking where A and $\sim A$ represent an attack and no attack, respectively. A mixed Markov strategy for S_1, $R_1(m_2)$, simply defines a probability distribution over the set of S_1's possible actions as a function of S_2's possible allocations. A Markov perfect equilibrium, in turn, is a pair of strategies or reaction functions (R_1, R_2) that constitute a subgame perfect equilibrium.[20]

I shall now develop three propositions. Proposition 1 identifies a condition that must be satisfied if a peaceful equilibrium is to exist; proposition 2 describes a pair of payoffs that strictly Pareto dominates any other feasible pair of peaceful equilibrium payoffs; and proposition 3 establishes conditions sufficient to ensure that these Pareto dominating peaceful payoffs can actually be attained in equilibrium and characterizes the unique peaceful Markov perfect equilibrium that yields them.

In establishing these results, it will be helpful to define the set of allocations (m_1^0, m_2^0) such that S_1 prefers—or is at least indifferent to—remaining at this allocation forever to attacking S_2 at m_2^0. To introduce some convenient notation, let $P_1(m_1)$ be S_1's payoff to devoting $r_1 - m_1$ to its internal ends in every period forever:

$$P_1(m_1) = \sum_{t=0}^{\infty} \delta_1^t (r_1 - m_1)^{\rho_1} = (r_1 - m_1)^{\rho_1}/(1 - \delta_1).$$

Also, let $W_1(m_1, m_2)$ be S_1's payoff to allocating m_1 to its military and attacking S_2 at m_2, that is $W_1(m_1, m_2) = F_1(0)$. The highest payoff that S_1 could attain if it attacks, given S_2's allocation of m_2, is $\overline{W}_1(m_2) = W_1(m_1^a(m_2), m_2)$, where m_1^a maximizes $W_1(m_1, m_2)$.[21] Given these definitions, the set of allocations at which S_1 prefers not to attack or is at least indifferent to attacking is $\mathcal{P}_1 = \{(m_1^0, m_2^0) : P_1(m_1^0) \geq \overline{W}_1(m_2^0)\}$.[22] The function $\tilde{m}_1(m_2)$ discussed informally is simply the set of allocations in $\mathcal{P}_1$ at which S_1 is indifferent between attacking; that is $\tilde{m}_1(m_2)$ satisfies $\overline{W}_1(m_2) = P_1(\tilde{m}_1(m_2))$ and is given by $\tilde{m}_1(m_2) = r_1 - ((1 - \delta_1)\overline{W}_1(m_2))^{1/\rho_1}$. The set $\mathcal{P}_2$ and the function $\tilde{m}_2(m_1)$ are defined similarly for S_2.

The present analysis will focus on the case in which there is a conflict of interest between the states and there is a potential reason to arm. Each state will be assumed to prefer attacking a disarmed adversary to devoting all of its resources to its internal ends—that is, S_1 prefers attacking a disarmed S_2 to devoting all of its resources to its domestic ends; and S_2 similarly prefers attacking a disarmed S_1. If there were no conflict of interest and both states preferred devoting all of their resources to their internal ends to attacking, then neither state would allocate anything to the military. More formally, a conflict of interest exists if S_1 strictly prefers to attack S_2 for all $m_2 < \varepsilon$ and S_2

strictly prefers to attack S_1 for all $m_1 < \gamma$, where ε and γ are small positive numbers.

Given the existence of a conflict of interest, the set $\mathcal{P}_1$ and the function $\tilde{m}_1(m_2)$ possess several useful properties. First, continuity ensures that $\mathcal{P}_1$ is closed and that $\tilde{m}_1(m_2)$ is continuous. Second, $\tilde{m}_1(m_2)$ is strictly increasing.[23] Finally, there exists some lower bound, $\underline{m}_2$, such that $\tilde{m}_1(\underline{m}_2) = 0$.[24] Similar properties hold for $\mathcal{P}_2$ and $\tilde{m}_2(m_1)$.

Turning to the statement of proposition 1, the two sets $\mathcal{P}_1$ and $\mathcal{P}_2$ establish a condition that must be satisfied if a peaceful equilibrium is to exist. By construction, S_1 prefers peace to war or is at least indifferent to it at all points (m_1, m_2) in $\mathcal{P}_1$; and S_2 prefers peace to war or is at least indifferent to it at all points in $\mathcal{P}_2$. Intuitively, a peaceful equilibrium can exist only if there are points that satisfy both of these conditions, that is, only if these two sets intersect. Figure 2 illustrates this intuition; and proposition 1, which is proved in the Appendix, formalizes it.

Proposition 1. *If there exists no allocation that both S_1 and S_2 prefer to accept forever over fighting to alter it, that is, if $\mathcal{P}_1 \cap \mathcal{P}_2 = \emptyset$, then there exist no peaceful equilibria.*

Suppose that these two sets do intersect, and let (m_1^*, m_2^*) be the lower tip of this intersection.[25] Clearly, any peaceful stationary equilibrium allocations must lie in this intersection. Thus, the payoffs associated with (m_1^*, m_2^*) strictly Pareto dominate the payoffs in any peaceful stationary equilibrium except, of course, the payoffs of the stationary equilibrium that has allocations (m_1^*, m_2^*) if such an equilibrium exists. In fact, the payoffs associated with (m_1^*, m_2^*) strictly Pareto dominate any peaceful equilibrium payoffs, whether stationary or not. This is formalized in proposition 2.

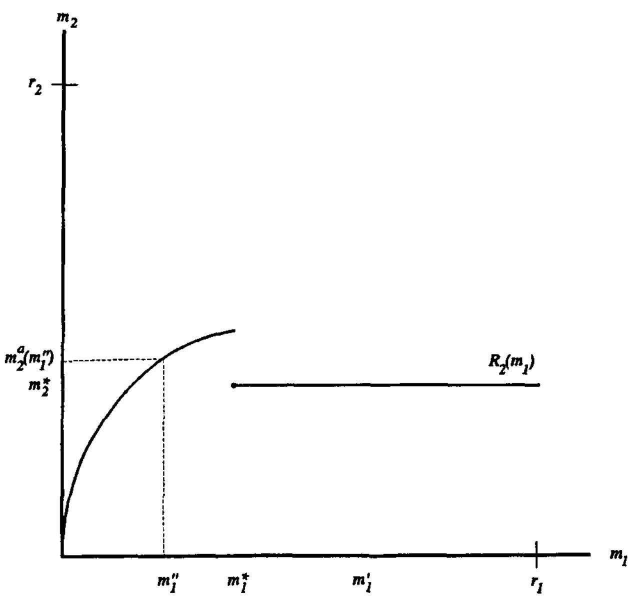
Proposition 2. *The combination of payoffs $(r_1 - m_1^*)^{\rho_1}/(1 - \delta_1)$ and $(r_2 - m_2^*)^{\rho_2}/(1 - \delta_2)$ strictly Pareto dominates any pair of peaceful equilibrium payoffs except itself.*

Proposition 2 demonstrates that neither state can attain a higher payoff in a peaceful equilibrium than the payoffs associated with the stationary allocation (m_1^*, m_2^*). Proposition 3 demonstrates that if one further assumption is made, then these payoffs can actually be attained in equilibrium. Indeed, a unique peaceful Markov perfect equilibrium yields them.

To specify the required condition, suppose that S_1 believed that war was inevitable at (m_1^*, m_2^*). In particular, assume that S_1 must decide what to do at (m_1^*, m_2^*), given that S_2 were certain to attack in the next period if S_1 does not attack in the current period. If S_1 decides to attack in these circumstances, it will make the optimal allocation $m_1^a(m_2^*)$ and strike. The payoff to this is $\overline{W}_1(m_2^*)$. If S_1 waits, the best it can attain is the maximum of $(r_1 - m_1)^{\rho_1} + \delta_1 W_1(m_1, m_2^a(m_1))$. The first term is the payoff to allocating $r_1 - m_1$ to its internal ends in the current period, and the second term is the expected utility to fighting S_2 at m_1 and at S_2's optimal allocation made in response to m_1.

FIGURE 4

S_2's Reaction Function



Conceivably, S_1 might prefer to wait; that is, S_1 in the face of an inevitable war might prefer to devote more to its internal ends today at the cost of having to fight on S_2's terms in the next period. More likely, however, S_1 will not want to reduce its forces if faced with a seemingly inevitable war. Formally, this means $\overline{W}_1(m_2^*) \geq \max_{\{m_1 \in [0,\, m_1^*]\}} \{(r_1 - m_1)^{\rho_1} + \delta_1 W_1(m_1, m_2^a(m_1))\}$.[26] If this condition holds, then there is a unique, peaceful Markov perfect equilibrium supporting the allocation (m_1^*, m_2^*).

Proposition 3. *If the states would not reduce their arms if war were inevitable at (m_1^*, m_2^*), then a unique peaceful Markov perfect equilibrium yields the military allocations (m_1^*, m_2^*) with the Pareto dominant payoffs $(r_1 - m_1^*)^{\rho_1}/(1 - \delta_1)$ and $(r_2 - m_2^*)^{\rho_2}/(1 - \delta_2)$. This equilibrium is given by*

$$R_1(m_2) = \begin{cases} (m_1^*, \sim A) & \text{if} \quad m_2 \geq m_2^* \\ (m_1^a(m_2)), A) & \text{if} \quad m_2 < m_2^* \end{cases}$$

$$R_2(m_1) = \begin{cases} (m_2^*, \sim A) & \text{if} \quad m_1 \geq m_1^* \\ (m_2^a(m_1)), A) & \text{if} \quad m_1 < m_1^*, \end{cases}$$

where $R_1(m_2)$ and $R_2(m_1)$ are the Markov reaction functions of S_1 and S_2, respectively.[27]

The reaction functions characterized in proposition 3, which is proved in the Appendix, are very simple. If S_1 allocates more to the military than m_1^*, say m_1' in Figure 4, then S_2 simply ignores this and continues to allocate m_2^* to the military. If S_1 ever allocates less to the military than m_1^*, say m_1'', S_2 reacts by attacking S_1 at the optimal attack allocation $m_2^a(m_1'')$.

The formal reason that S_2 does not react to an $m_1' \geq m_1^*$ is that in equilibrium, S_2 expects S_1 to follow R_1 even after the deviation to m_2'. Thus, S_2 will receive

$(r_1 - m_1^*)^{\rho_2}/(1 - \delta_2)$ if it continues m_2^*. If it allocates more than m_2^* and does not attack, then according to R_1, S_1 will ignore this and allocate m_1^*, leaving S_2 with less than $(r_1 - m_1^*)^{\rho_2}/(1 - \delta_2)$. If S_2 allocates less than m_2^* to defense, S_1 will then attack and the assumption just made implies that the payoff to this is less than $(r_1 - m_1^*)^{\rho_2}/(1 - \delta_2)$. Finally, if S_2 attacked S_1 at m_1', it would do so optimally. But the payoff to this is worse than $(r_1 - m_1^*)^{\rho_2}/(1 - \delta_2)$.[28] In short, S_2's best reply to $m_1' \geq m_1^*$ is to ignore it.

If S_1 allocates less to the military than m_1^*, then S_2 attacks. It might at first seem that S_2 could reduce its allocation, rather than attack, and thereby leave both S_1 and S_2 better off. Alas, if S_2 attacks S_1 at $m_1'' < m_1^*$, it will do better than it can do in any peaceful equilibrium. Attacking brings $\overline{W}_2(m_1'')$, which is greater than $P_2(m_2^*)$.

Finally, note that the probability that a state will prevail in the event of war is a function of the relative sizes of the states' military allocations. The probability of a successful attack is increasing in the attacker's military allocation and decreasing in the defender's military allocation. This, in turn, might therefore be expected to result in upwardly sloped reaction functions in which each state increases its military allocation in response to the other's increase. Proposition 3 shows that this is not the case. Neither state responds to an increase in the other's military allocation with an increase in its own. $R_1(m_2) = m_1^*$ for all $m_2 \geq m_2^*$ and $R_2(m_1) = m_2^*$ for all $m_1 \geq m_1^*$. There are no arms races in this model.

This insensitivity appears to be an artifact of the model's very simple structure. If, for example, the states' could only reallocate a fraction of their resources (rather than all of them, as in the present formulation), then upwardly sloped reaction functions seem likely. Or if there were incomplete information, then the states might not be insensitive to increases in the others' military allocation. Richer models developed along these lines may provide a more satisfactory setting for the analysis of arms races.

In short, a unique, peaceful Markov perfect equilibrium supports the payoffs $(r_1 - m_2^*)^{\rho_1}/(1 - \delta_1)$ and $(r_2 - m_2^*)^{\rho_2}/(1 - \delta_2)$. In equilibrium, the states ignore allocations above (m_1^*, m_2^*) and attack when this offers a higher payoff than would staying at (m_1^*, m_2^*) forever. Finally, the payoffs in this peaceful Markov perfect equilibrium strictly Pareto dominate any other peaceful equilibrium payoffs.

THE MODEL AND INTERNATIONAL RELATIONS THEORY

The comparative static analysis of the model illuminates some specific issues. But the model also touches on broader concerns in international relations theory. It qualifies some of the results of the recent formal work on the theory of the balance of power. The model also questions the meaning or significance of several concepts (e.g., anarchy and the difference between absolute and relative gains) that have been central to many theories of international relations.

For Waltz, "If there is any distinctively political theory of international politics, balance of power theory is it. And yet one cannot find a statement of it that is generally accepted" (Waltz 1979, 117). Building on the work of Wagner (1986), Niou and Ordeshook (1990, 1991) have used noncooperative game theory to study this theory. In their model, there are n states, $S_1, S_2, \ldots, S_n$, and S_k begins the game with resources r_k. The total amount of resources in the system remains constant at

$$r = \sum_{k=1}^{n} r_k$$

throughout the game. Resources are never consumed and may only be redistributed among the states. This can occur solely as the result of peaceful transfers or through war. Each state in this infinite game tries to maximize the resources it ultimately controls.

A main result is that if all states in the international system are essential[29] and if the states follow stationary strategies, then the system is stable; that is, no essential state will be eliminated (Niou and Ordeshook 1990, 1224). Inessential states may, however, be eliminated (pp. 1228–29). Indeed, in the special case of a bipolar system, one of the states will always be eliminated unless the two states' resources are exactly the same, that is, unless $r_1 = r_2$.

The present analysis gives a different result. The functions $\tilde{m}_1$ and $\tilde{m}_2$ intersect and a peaceful Markov perfect equilibrium exists for many cases in which $r_1 \neq r_2$.[30] The explanation for this difference lies in part in the different assumptions the models make about state preferences and the relation between the states' military allocations and the probability of prevailing. In the Niou and Ordeshook model, there is no trade-off between internal ends and military means. A state's utility is an explicit function of the resources it controls; at the same time, these resources define the state's power. The states in this model are effectively trying to maximize their power. But for the reasons already elaborated, the present model has been deliberately designed to treat power very much as a means, not as an end in itself. Accordingly, the model explicitly represents the trade-off between internal ends and instrumentally valued military means that acquire their instrumental value because of the strategic setting of the international system.

These different assumptions about state preferences force the models to focus on different types of balancing. States may engage in external balancing in which they try to enhance their security through alliances with others, or states may try to balance internally by building up their own capabilities (Waltz 1979, 168). In the Niou and Ordeshook model, states cannot balance internally by building up their own capabilities; they can only balance externally through coalitions or by defeating their adversaries and absorbing their resources. Conversely, states in the model analyzed here cannot balance externally, because there are only two states. Thus, the states must balance internally; for there are no other states with which to ally.

Niou and Ordeshook (1990) also make a different assumption about the relation between the states' military allocations, which in their model are equivalent to the states' resources, and the probability of prevailing. If a state with more resources attacks a state with fewer resources, the stronger state is certain to prevail (p. 1209). Even if a state's resources are only slightly greater than another state's, the former is certain to certain to defeat the latter and completely absorb its resources. In contrast, the present model assumes that the probability that a state will prevail varies smoothly with the state's military allocations. States with similar but not quite equal allocations have similar probabilities of prevailing.[31] In order for a potential attacker to raise its probability of success significantly, it must significantly increase its military allocation relative to the other state.[32]

Given the different focuses of these models, it might seem that these two models might be treated as complements until a more general formulation can be developed. This, however, is problematic. As noted, these models give very different results in areas in which they may both be applied, for instance, in the case of a bipolar international system.

Much recent work in international relations theory has focused on the implications of anarchy and on the related problem of absolute and relative gains (Gilpin 1981; Gowa 1986; Grieco 1988a, 1988b, 1990; Jervis 1988; Keohane 1984; Mearsheimer 1990; Milner 1991; Oye 1986; Powell 1991; Snidal 1990; Waltz 1959, 1979, 1990). The model developed here casts doubt on the substantive significance of the concept of anarchy and on the importance of the distinction between absolute and relative gains.

Anarchy is often taken to be "the fundamental fact of international relations" (Art and Jervis 1986, 7) and "the characteristic that distinguishes international politics from ordinary politics" (Wight 1978, 102). Indeed, the notion of anarchy has served as a central organizing concept for many theoretical approaches to the study of international politics. But notwithstanding its presumed theoretical importance, "anarchy" is difficult to define precisely and what explicit and implicit definitions there are often differ.[33]

Two definitions will be considered here. The first, which is widely used in international political economy is that anarchy means the lack of a central authority that can enforce agreements among states or, more generally, among the actors in the system (Axelrod and Keohane 1986, 226–27; Oye 1986, 1–2). The absence of a central authority implies that the system is one of "self-help," in which any agreements must be self-enforcing and that threats if they are to be effective must be credible in the sense that actually carrying out the threat if called on to do so will be in the threatener's self-interest (Waltz 1979, 88–93). The second definition, which may initially seem to be more in keeping with structural realism's emphasis on the importance of force and power in international politics, adds a second dimension to the absence of a central authority. Here, anarchy is the lack of a central authority in a strategic setting in which the threat of force is omnipresent.

The present analysis suggests that if the first definition is adopted, then anarchy loses any substantive significance that is distinctively related to international politics. Anarchy cannot be "the characteristic that distinguishes international politics from ordinary politics" (Wight 1978, 102). The only way that the lack of a central authority entered the analysis is that the model was solved for perfect equilibria, rather than simply Nash equilibria. In a perfect equilibrium, unlike a Nash equilibrium, actors cannot rely on threats that would not be in their own interest to carry out if circumstances eventually called on them to do so. Thus, solving for perfect equilibria formalizes the lack of a central authority.

But this identification of anarchy with solving for perfect equilibria largely deprives the notion of anarchy of any substantive significance distinctively related to international politics, for an extremely diverse set of substantive problems are being formalized game-theoretically and analyzed in terms of perfect equilibria. Examples include the bargaining that takes place between an individual buyer and seller (Fudenberg and Tirole 1983), the use of price and advertising to signal the quality of a new product (Milgrom and Roberts 1986), and the effects of institutional structure on legislative bargaining (Baron and Ferejohn 1989). This wide range of substantive applications of the concept of perfect equilibria robs anarchy of any substantive importance distinctively related to international politics. Indeed, describing the various strategic settings underlying these problems as anarchic adds little to a better understanding of them. If anything is substantively distinctive about international politics, it is not anarchy (at least under the first definition).

Consider, now, the second definition. There are two things to be noted. First, although adding the second dimension of an omnipresent threat of force may seem to bring the definition of anarchy closer to the notion implicit in much of realism, this definition is incompatible with Waltz's (1979) concept of an anarchic political structure. To wit, Waltz describes many realms as anarchic in which the actors, while posing a threat to each other, are not threatening to use force. For example, an oligopolistic market in which the firms face a high risk of bankruptcy may be anarchic (p. 111).

Second, if anarchy defined in this second way really does distinguish international politics, then the distinctive characteristic is not the lack of a central authority but the means of coercion (i.e., force) that is available to the actors. But what, then, is distinctive about force as a means of coercion? In more narrow game-theoretic terms, does a model in which the actors are states and force is the means of coercion differ in important ways from a model of an oligopolistic market in which the actors are firms and the means of coercion is cutting prices and expanding output? In my view, this question, as well as the broader question of what if anything is distinctive about international politics, is open.

As noted, what to assume about states' preferences has been a contentious issue in international relations theory. Indeed, one of the fundamental differences between neoliberal institutionalism and structural realism is often cast in terms of what these two theoretical approaches assume about state preferences (Gilpin 1981, 18–25; Gowa 1986, 172–79; Grieco 1988a, 1988b, 1990; Jervis 1988, 334–36; Keohane 1984; Krasner 1983, 356). Neoliberal institutionalism, which is often used to study topics in international political economy, assumes that states are primarily concerned with their own absolute gains and are indifferent to the gains of others. Structural realism, which is usually used to analyze security issues, argues that the anarchic nature of the international system forces states to be concerned with relative, rather than absolute gains (e.g., Waltz 1959, 1979). Expressing this difference in terms of utility functions, neoliberal institutionalism takes a state's utility to be solely a function of its absolute gain while structural realism takes a state's utility to be at least partly a function of some relative measure like power. The strong claim implicit in distinguishing between these two theoretical approaches in terms of their assumptions about state preferences is that one cannot develop a model that is, for example, compatible with structural realism in which the states are maximizing their absolute gains.

I have elsewhere suggested that discussing the problem of relative and absolute gains and casting the differences between structural realism and neoliberal institutionalism in terms of different assumptions about state preferences is misguided (Powell 1991). The present analysis reinforces that point in the context of a more interesting and less contrived model. The states in the present model are maximizing their absolute gains, each state's utility being solely a function of its own internal allocations. But the model is clearly in keeping with structural realism: the primary threat to a state's utility is that another state will use force against it. Thus, the model demonstrates that structural realism is quite compatible with the assumption that states are concerned with their absolute gains. Whatever the differences between these two theoretical approaches may be, they do not turn on making different assumptions about state preferences.

CONCLUSION

The international system confronts states with a trade-off between intrinsically valued ends and the means of power. This guns-versus-butter problem has been formalized as a simple infinite-horizon game. Increases in the expected payoff to fighting and a state's willingness to run risks, as well as shifts in the offense–defense balance toward the offense, lead to higher military allocations. In contrast to other work, the shadow of the future does not promote greater welfare. The longer the shadow of the future (i.e., the higher the states' discount factors), the higher their military allocations and the lower their payoffs. The analysis has also cast doubt on the substantive significance of anarchy and of the related problem of absolute and relative gains.

Analogies between oligopolies and the international system are central to many theories of international relations. But the formal economic models of oligopoly generally lack formal counterparts in international relations theory. I have developed and analyzed a simple model of a bipolar international system.

The analysis of this model contributes to international relations theory in five ways. First, it helps to ground some aspects of international relations theory in oligopoly theory, or, more accurately, game theory. Second, it also helps to clarify the relations among the states' willingness to take risks, the shadow of the future, the offense–defense balance, the states' military allocations, and stability. It shows, in particular, that the equilibrium military allocations rise if the payoff to prevailing in the event of war increases, the cost of fighting falls, the shadow of the future looms larger in that the states' discount factors rise, the states become more risk-acceptant, and the offense–defense balance more and more favors the offense. Third, the model points toward a more satisfactory arms race model. Fourth, it indicates that some problems that have been at the center of much debate in international relations theory (e.g., the notion of anarchy and the problem of relative gains) turn out to be relatively unimportant once they are examined in the context of a formal model in which the relevant concepts can be defined more clearly and precisely. Finally and most importantly, the model shifts attention away from broad analogies and general notions to a much more focused consideration of the constraints confronting the states in the international system.

Appendix

Proof of Proposition 1. The proof proceeds by contradiction. Assume that a peaceful equilibrium exists and that $\mathcal{P}_1$ and $\mathcal{P}_2$ do not intersect. This will be shown to lead to a contradiction.

The first step is to describe two properties of $\tilde{m}_1$ and $\tilde{m}_2$. First, because $\tilde{m}_1$ and $\tilde{m}_2$ do not intersect, $\tilde{m}_1^{-1}(m_1)$ lies above $\tilde{m}_2(m_1)$ and $\tilde{m}_2^{-1}(m_2)$ lies above $\tilde{m}_1(m_2)$, that is, $\tilde{m}_1^{-1}(m_1) > \tilde{m}_2(m_1)$ and $\tilde{m}_2^{-1}(m_2) > \tilde{m}_1(m_2)$. To show the former, recall that $\underline{m}_1$ is defined so that $\tilde{m}_2(\underline{m}_1) = 0$. Furthermore, $\tilde{m}_1^{-1}(m_1)$ is increasing in m_1 and $\tilde{m}_1^{-1}(0) = \underline{m}_2$. This means $\tilde{m}_1^{-1}(\underline{m}_1) > \tilde{m}_1^{-1}(0) = \underline{m}_2 > 0 = \tilde{m}_2(\underline{m}_1)$. Thus, $\tilde{m}_1^{-1}$ is above $\tilde{m}_2$ at $\underline{m}_1$. But then the continuity of $\tilde{m}_1^{-1}$ and $\tilde{m}_2$ and the assumption that $\mathcal{P}_1$ and $\mathcal{P}_2$ do not intersect ensures that $\tilde{m}_1^{-1}$ is always above $\tilde{m}_2$. A similar argument shows that $\tilde{m}_2^{-1}$ is always above $\tilde{m}_1$. The second property is that either $\tilde{m}_1(r_2) < r_1$ or $\tilde{m}_2(r_1) < r_2$.

Otherwise, $\mathcal{P}_1$ and $\mathcal{P}_2$ would intersect at (r_1, r_2). Without loss of generality, $\tilde{m}_1(r_2)$ will be assumed to be less than r_1.

Now let (σ_1^*, σ_2^*) be a peaceful Nash equilibrium. With probability 1, S_1 will have to respond to some allocation in the interval $[\underline{m}_2, r_2]$. This interval is bounded below by $\underline{m}_2$, because S_2 can never allocate less to the military than $\underline{m}_2$ in a peaceful equilibrium. To see this, note that the best that S_1 could possibly do in a peaceful equilibrium is to devote all of its resources to its domestic ends in each period, which would give it $P_1(0)$. But if S_1 optimally attacks S_2 at any $m_2 < \underline{m}_2$, it receives $\overline{W}_1(m_2)$, which is greater than $P_1(0)$ because $m_2 < \underline{m}_2$. Thus, S_1 will have an incentive to deviate from any peaceful equilibrium strategy if S_1's allocation is ever less than $\underline{m}_2$. But no actor can have a positive incentive to deviate from an equilibrium strategy, so it must be that S_2's military allocation is never less than $\underline{m}_2$ in a peaceful equilibrium.

If S_1 attacked at any $m_2 \in [\underline{m}_2, r_2]$, then S_1 would, by construction, receive $P_1(\tilde{m}_1(m_2))$. But, $P_1(\tilde{m}_1(m_2))$ is decreasing in m_2, so S_1 is certain to attain at least $P_1((\tilde{m}_1(r_2))$ by attacking. But S_1 does not attack in a peaceful equilibrium, so S_1 must attain at least $P_1((\tilde{m}_1(r_2))$ by not attacking. The only condition under which S_1 can achieve this payoff is if there is a positive probability that S_1 will play an allocation less than or equal to $\tilde{m}_1(r_2)$. Let m_1^0 denote $\tilde{m}_1(r_2)$ for notational convenience. S_1's allocation is also bounded below in a peaceful equilibrium by $\underline{m}_1$ for the same reason that S_2's allocations are bounded below by $\underline{m}_2$. Thus, S_1 must play some $m_1 \in [\underline{m}_1, \tilde{m}_1(r_2)]$ with positive probability.

The payoff S_2 will receive by attacking S_1 at any $m_1 \in [\underline{m}_1, \tilde{m}_1(r_2)]$ is bounded below by $P_2(\tilde{m}_2(\tilde{m}_1(r_2)))$. To deter S_2 from attacking in equilibrium, S_2 must therefore receive at least this lower bound, which is possible only if S_2's allocation lies in the interval $[\underline{m}_2, \tilde{m}_2(\tilde{m}_1(r_2))]$ in some period with positive probability. The upper bound of this interval is, in turn, strictly less than r_2, because $\tilde{m}_1^{-1}(m_1)$ lies above $\tilde{m}_2(m_1)$.

But for any $m_2 \in [\underline{m}_2, \tilde{m}_2(\tilde{m}_1(r_2))]$, S_1 will attain at least $P_1(\tilde{m}_2(\tilde{m}_1(r_2)))$ by attacking. Thus, S_1 must receive at least this much by not attacking, which implies that it must play some point in the interval $[\underline{m}_1, \tilde{m}_1(m_2(\tilde{m}_1(r_2)))]$ with positive probability. The upper bound of this interval is strictly less than $\tilde{m}_1(r_2)$, because $\tilde{m}_2^{-1}(m_2)$ lies above $\tilde{m}_1(m_2)$, so $\tilde{m}_1(\tilde{m}_2(\tilde{m}_1(r_2))) < \tilde{m}_2^{-1}(\tilde{m}_2(\tilde{m}_1(r_2))) = \tilde{m}_1(r_2)$.

Once again, S_2 can attain at least $P_2(\tilde{m}_2(\tilde{m}_1(\tilde{m}_2(\tilde{m}_1(r_2)))))$ by attacking S_1 in the interval $[\underline{m}_1, \tilde{m}_1(\tilde{m}_2(\tilde{m}_1(r_2)))]$. To achieve this much in a peaceful equilibrium, S_2 must play some allocation in the interval $[\underline{m}_2, \tilde{m}_2(\tilde{m}_1(\tilde{m}_2(\tilde{m}_1(r_2))))]$ with some positive probability. The upper bound of this interval also satisfies $\tilde{m}_2(\tilde{m}_1(\tilde{m}_2(\tilde{m}_1(r_2)))) < \tilde{m}_2(\tilde{m}_1(r_2)) < r_2$.

Continuing to consider the decisions facing S_1 and S_2 in this way generates the sequence $Q_1 = \{m_1^0, \tilde{m}_1(m_1^0), \tilde{m}_1(\tilde{m}_2(\tilde{m}_1(m_1^0))), \tilde{m}_1(\tilde{m}_2(\tilde{m}_1(\tilde{m}_2(\tilde{m}_1(m_1^0))))), \ldots\}$, which is well defined, strictly decreasing, and bounded below by $\underline{m}_1$. Thus, Q_1 converges to some $m_1^* \geq \underline{m}_1 > 0$. Now form the new sequence Q_2 from Q_1 by applying $\tilde{m}_2$ to each element of Q_2. Because each element of Q_1 is greater that $\underline{m}_1 > 0$, Q_2 is well defined. The continuity of $\tilde{m}_2$ also assures that this new sequence converges to $m_2^* = \tilde{m}_2(m_1^*)$. But this leads to a contradiction, for $\tilde{m}_1$ and $\tilde{m}_2$ intersect at (m_1^*, m_2^*). Suppose that they did not intersect. Then, $m_1^* \neq \tilde{m}_1(m_2^*)$. But except for its first two elements, Q_1 may be obtained from Q_2 by applying $\tilde{m}_1$ to each element of Q_2. Thus, the continuity of $\tilde{m}_1$ gives $m_1^* = \tilde{m}_1(m_2^*)$. This contradiction establishes the proposition. ‖

Proof of Proposition 2. The first step is to define the lower tip of the intersection of $\mathcal{P}_1 \cap \mathcal{P}_2$, which is denoted by (m_1^*, m_2^*). Take m_1^* to be the minimum value of m_1 such that there exists some m_2 such that $(m_1, m_2) \in \mathcal{P}_1 \cap \mathcal{P}_2$. This intersection is closed, so m_1^* is certain to exist. Define m_2^* analogously. To see that (m_1^*, m_2^*) is an element of $\mathcal{P}_1 \cap \mathcal{P}_2$, let m_2^0 be the value of m_2 such that $(m_1^*, m_2^0) \in \mathcal{P}_1 \cap \mathcal{P}_2$. Now $\tilde{m}_1^{-1}(m_1)$ is strictly increasing in m_1. If, therefore, $m_1' > m_1^*$ and $(m_1', m_2') \in \mathcal{P}_1 \cap \mathcal{P}_2$, then $m_2' \geq \tilde{m}_1^{-1}(m_1') > m_2^0$. Hence, m_2^0 is the minimum m_2 such that there exists a point with this value of m_2 in $\mathcal{P}_1 \cap \mathcal{P}_2$; that is, $m_2^0 = m_2^*$.

To establish that the combination of payoffs at (m_1^*, m_2^*) Pareto dominates any pair of peaceful equilibrium payoffs (except possibly itself), assume that one state, say S_1, could do better than $P_1(m_1^*)$ in some peaceful equilibrium (σ_1^*, σ_2^*). To do strictly better, there must be some positive probability that at some time S_1's allocation will lie in the interval $[\underline{m}_1, m_1^0]$ where $m_1^0 < m_1^*$. Consequently, S_2 must be able to achieve at least $P_2(\tilde{m}_2(m_1^0))$ by not attacking, since this is a lower bound on what could be achieved by attacking.

By construction, $\mathcal{P}_1$ and $\mathcal{P}_2$ do not intersect and $\tilde{m}_1^{-1}(m_1)$ lies above $\tilde{m}_2(m_1)$ in the set $[0, m_1^*) \times [0, m_2^*)$. Accordingly, the argument used in the proof of proposition 1 may now be evoked to show that $\tilde{m}_1$ and $\tilde{m}_2$ must intersect in this region, a contradiction that establishes the proposition. ‖

Proof of Proposition 3. The reaction functions R_1 and R_2 clearly satisfy the Markov property and are peaceful as long as the initial exogenous allocation is at least m_2^*. It will therefore suffice to show that (R_1, R_2) constitutes a subgame perfect equilibrium and that it is the unique peaceful Markov perfect equilibrium supporting the payoffs $P_1(m_1^*)$ and $P_2(m_2^*)$.

To establish subgame perfection, it will suffice to show that neither player can improve its payoff by deviating from its reaction function in one period and then conforming it in all subsequent periods. (See Fudenberg and Tirole 1991, 110, for a proof of this in multistage games with finite action spaces. Their argument applies equally well to the current game, which has a continuum of actions.) Without loss of generality, consider any subgame beginning with S_1's having to decide what to do with S_2's allocation being $m_2^0 \geq m_2^*$. Following R_1, given that S_2 will follow R_2, brings S_1 the payoff $P_1(m_1^*)$. To see that S_1 cannot improve its payoff by attacking in this period, suppose that S_1 did attack immediately. It would receive $\overline{W}_1(m_2^0)$ at most. But, $\overline{W}_1(m_2^0) \leq P_1(m_1^*)$, because $\overline{W}_1(m_2^0) = P_1(\tilde{m}_1(m_2^0))$, $m_2^0 \geq m_2^*$, and $\tilde{m}_1$ is increasing. Thus, S_1 has no incentive to attack immediately. It also has no incentive to deviate from R_1 by allocating more than m_1^* and then conforming to R_1 in the future. If S_1 allocates $m_1^0 > m_1^*$ to the military, its payoff is $(r_1 - m_1^0)^{\rho_1} + [\delta_1/(1 - \delta_1)]P_1(m_1^*) < P_1(m_1^*)$. If S_1 allocated less than m_1^* and did not attack, then its payoff will still be less than $P_1(m_1^*)$ because S_2 will then attack, which, by assumption, will leave S_1 worse off than $P_1(m_1^*)$.

Now consider a subgame in which $m_2^0 < m_2^*$ and S_1 must decide what to do. Following R_1 by optimally attacking S_2 brings $\overline{W}_1(m_2^0)$, which is greater than $P_1(m_1^*)$ because $m_2^0 < m_2^*$, $\tilde{m}_1$ is increasing, and $\overline{W}_1(m_2^0) = (r_1 - \tilde{m}_1(m_0^2))^{\rho_1}/(1 - \delta_1)$. If S_1 deviates to some $m_1 \geq m_1^*$ without attacking and then conforms to R_1 thereafter, it receives $(r_1 - m_1^0)^{\rho_1} + [\delta_1/(1 - \delta_1)]P_1(m_1^*) < P_1(m_1^*)$. Accordingly, S_1 has no incentive to deviate in this way. If S_1 deviates to some $m_1 < m_1^*$ and does not attack, it must maximize $(r_1 - m_1)^{\rho_1} + \underline{\delta_1} W_1(m_1, m_2^a(m_1))$. But by assumption, this is no more than $\overline{W}_1(m_2^*)$ as long as $m_2^0 < m_2^*$. Hence, S_1 has no incentive to deviate in this way. In short, no player has any incentive to deviate from its reaction function in a single period and conform to it there after, which establishes that (R_1, R_2) is subgame perfect.

It remains to be shown that (R_1, R_2) is the only peaceful Markov perfect equilibrium supporting the payoffs $P_1(m_1^*)$ and $P_2(m_2^*)$. Let (R_1', R_2') be any peaceful Markov perfect equilibrium. It will suffice to show $R_1 = R_1'$ and $R_2 = R_2'$.

The first step is to demonstrate that S_1 is certain to attack at all $m_2 < m_2^*$. Suppose that there is some positive probability that S_1 will not attack at some $m_2^0 < m_2^*$. The equilibrium payoff to not attacking at m_2^0 must be at least as large as the payoff to attacking, $\overline{W}_1(m_2^0)$; that is, there must be some m_1^0 such that S_1's payoff to playing m_1^0 is at least $\overline{W}_1(m_2^0)$. But $\overline{W}_1(m_2^0 > P_1(m_1^*)$, so the payoff to playing m_1^0 is greater than $P_1(m_1^*)$. This, however, immediately leads to a contradiction; for S_1 can now do better than its purported equilibrium payoff of $P_1(m_1^*)$ by simply playing m_1^0 at the beginning of the game. Thus, $R_1(m_2) = R_1'(m_2)$ for $m_2 < m_2^*$. A similar argument gives $R_2(m_1) = R_2'(m_1)$ for $m_1 < m_1^*$.

Now suppose that S_1 attacks with some positive probability at m_2^* according to $R_1'(m_2)$. Then S_2 must always allocate $m_2 > m_2^*$ in $(R_1'(m_2), R_2'(m_1))$ because this equilibrium is peaceful. But these allocations give S_2 a payoff strictly less $P_2(m_2^*)$, which is a contradiction. Hence, S_1 does not attack at m_1^* in $R_1'(m_2)$. Similarly, S_2 does not attack at m_2^* in $R_2'(m_1)$.

Consider now S_1's reaction to some $m_2 \geq m_2^*$. As just shown, S_1 will not attack. If S_1 reacts to $m_2 \geq m_2^*$ with some $m_1^0 < m_1^*$, then S_2 will attack. This will leave S_1 with $(\underline{r_1} - m_1^0)^{\rho_1} + \delta_1 W_1(m_1^0, m_2^a(m_1^0))$. By assumption, this is less than $\overline{W}_1(m_2^*) = P_1(m_1^*)$. This means that $(m_1^0, {\sim}A)$ is not a best reply to R_2'. Hence, S_1 will not react to $m_2 \geq m_2^*$ with some $m_1^0 < m_1^*$. Similarly, S_2 will not react to $m_1 \geq m_1^*$ with some $m_2^0 < m_2^*$.

Since neither state attacks as long as $(m_1, m_2) \in [m_1^*, r_1] \times [m_2^*, r_2]$ and neither will move out of this rectangle, the equilibrium path of any subgame beginning in this rectangle must remain there. But then S_1 can achieve $P_1(m_1^*)$, which it must do in (R_1', R_2'), only if S_1 always allocates m_1^*; that is, $R_1'(m_2)$ for $m_2 \geq m_2^*$ must be $(m_1^*, {\sim}A)$. A similar argument holds for S_2. This leaves $R_1 = R_1'$ and $R_2 = R_2'$. ‖

Comparative Statics. The effects of changes in the values of the model's parameters can generally be analyzed in terms of their effects on $\tilde{m}_1$ and $\tilde{m}_2$. For example, anything that shifts $\tilde{m}_2$ upward or $\tilde{m}_1$ to the right shifts the intersection of $\tilde{m}_1$ and $\tilde{m}_2$ down and to the left, because $\tilde{m}_1$ and $\tilde{m}_2$ are increasing in m_2 and m_1, respectively, and $\tilde{m}_2$ cuts $\tilde{m}_1$ from below at (m_1^*, m_2^*). This leads to lower military allocations and higher internal allocations. In tracing the effects of changes in the values in v, it is easy to see that $W_1(m_1, m_2)$ is increasing in v_1 and therefore that $\overline{W}_1(m_2; v_1)$ is increasing in v_1. But $\tilde{m}_1(m_2)$ is decreasing in $\overline{W}_1$, so $\tilde{m}_1(m_2; v_1')$ is decreasing in v_1. A similar analysis shows that an increase in d_1 shifts $\tilde{m}_1$ in and that an increase in ω shifts both $\tilde{m}_1$ in and $\tilde{m}_2$ down. To establish that an increase in the discount rate leads to greater military allocations, it will suffice to show that S_1 strictly prefers to fight along $\tilde{m}_1(m_2; \delta_1^0)$ for any $\delta_1' > \delta_1^0$; that is, $\overline{W}_1(m_2; \delta_1') > P_1(\tilde{m}_1(m_2; \delta_1^0); \delta_1')$. But

$$\begin{aligned}
&\overline{W}_1(m_2; \delta_1') - P_1(\tilde{m}_1(m_2; \delta_1^0); \delta_1') \\
&\quad \geq W_1(m_1^a(m_2; \delta_1^0), m_2; \delta_1') - P_1(\tilde{m}_1(m_2; \delta_1^0); \delta_1') \\
&\quad \geq (r_1 - m_1^a(m_2; \delta_1^0))^{\rho_1} - (r_1 - \tilde{m}_1(m_2; \delta_1^0))^{\rho_1} \\
&\qquad + \frac{\delta_1'}{1 - \delta_1'} [\pi(m_1^a(m_2; \delta_1^0), m_2) v_1^{\rho_1} \\
&\qquad + (1 - \pi(m_1^a(m_2; \delta_1^0), m_2)) d_1^{\rho_1} - (r_1 - \tilde{m}_1(m_2; \delta_1^0))^{\rho_1}],
\end{aligned}$$

where the latter inequality follows from the definition of $W_1(m_1^a(m_2; \delta_1^0), m_2; \delta_1')$. Now by assumption, S_1 would increase its arms if it were to attack; that is, $m_1^a(m_2; \delta_1^0)$ is greater than $\tilde{m}_1(m_2; \delta_1^0)$. This means that the difference between the first two terms on the right side of the last inequality is negative. Thus, the coefficient of $\delta_1'/(1 - \delta_1')$ must be positive, because the entire expression is zero if evaluated at $\delta_1' = \delta_1^0$. The positive coefficient implies that the entire expression on the right side of the inequality is positive for $\delta_1' > \delta_1^0$, and this establishes the result.

To show that $\tilde{m}_1(m_2)$ is decreasing in ρ_1, let $m_1^0 = \tilde{m}_1(m_2^0; \rho_1^0)$. Taking $\rho_1' > \rho_1^0$, it is sufficient to show that $\overline{W}_1(m_2^0; \rho_1') > (r_1 - m_1^0)\rho_1'/(1 - \delta_1)$; for that inequality and the relation $\overline{W}_1(m_2^0; \rho_1') = P_1(\tilde{m}_1(m_2^0; \rho_1'))$ give $\tilde{m}_1(m_2^0; \rho_1') < \tilde{m}_1(m_2^0; \rho_1^0)$. By definition, $\overline{W}_1(m_2^0; \rho_1') \geq W_1(m_1^a(m_2^0; \rho_1^0), m_2^0; \rho_1')$, so it will suffice demonstrate that $W_1(m_1^a(m_2^0; \rho_1^0), m_2^0; \rho_1') > P_1(m_1^0; \rho_1')$. This last inequality is equivalent to

$$\begin{aligned}
\frac{(r_1 - m_1^0)^{\rho_1'}}{1 - \delta_1} &< (r_1 - m_1^a(m_2^0; \rho_1^0))^{\rho_1'} \\
&\quad + \frac{\delta_1}{1 - \delta_1} (\pi(m_1^a(m_2^0; \rho_1^0), m_2^0, \omega) v_1^{\rho_1'} \\
&\quad + (1 - \pi(m_1^a(m_2^0; \rho_1^0), m_2^0, \omega)\, d_1^{\rho_1'}).
\end{aligned}$$

Multiplying through by $1 - \delta_1$ and raising to the power of ρ_1^0/ρ_1' gives

$$\begin{aligned}
(r_1 - m_1^0)^{\rho_1^0} &< [(1 - \delta_1)(r_1 - m_1^a(m_2^0; \rho_1^0))^{\rho_1'} \\
&\quad + \delta_1(\pi(m_1^a(m_2^0; \rho_1^0), m_2^0, \omega) v_1^{\rho_1'} \\
&\quad + (1 - \pi(m_1^a(m_2^0; \rho_1^0), m_2^0, \omega)\, d_1^{\rho_1'})]^{\rho_1^0/\rho_1'}.
\end{aligned}$$

Given that $p_1^0/\rho_1' < 1$, the function $g(y) = y^{\rho_1^0/\rho_1'}$ is strictly concave downward. Using this fact twice means that it is now enough to show

$$\begin{aligned}
(r_1 - m_1^0)^{\rho_1^0} &\leq (1 - \delta_1)(r_1 - m_1^a(m_2^0; \rho_1^0))^{\rho_1^0} \\
&\quad + \delta_1 \Big(\pi(m_1^a(m_2^0; \rho_1^0), m_2^0, \omega) v_1^{\rho_1^0} \\
&\quad + (1 - \pi(m_1^a(m_2^0; \rho_1^0), m_2^0, \omega)\, d_1^{\rho_1^0}) \Big).
\end{aligned}$$

But this inequality is simply $P_1(\tilde{m}_1(m_2^0; \rho_1^0)) \leq \overline{W}_1(m_2^0; \rho_1^0)$, which is true by the definition of $\tilde{m}_1$. ‖

Notes

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1. The assumption that a state acquires no direct benefits from military allocations is, of course, not intended to suggest that states do not actually gain some direct benefits from military spending. Rather, this assumption is intended to abstract away from these benefits so as to focus on the trade-off between domestic ends and military means that acquire instrumental value in the international system.

2. As will be developed more fully below, the formal model's focus on this trade-off distinguishes this formulation in an important way from several other formal models in international relations theory (Niou and Ordeshook 1986, 1987, 1990, 1991; Niou, Ordeshook, and Rose 1989; Wagner 1986). See, however, Brito and Intriligator 1985 for a formal model centering explicitly on this dilemma.

3. For discussions of neoliberal institutionalism's emphasis on absolute gains and realism's or structural realism's emphasis on relative gains, see Gilpin 1981, 18–25; Gowa 1986, 172–79; Grieco 1988a, 1988b, 1990; Hoffmann, 1973, 6–12; Jervis 1988, 334–36; Keohane 1984; Keohane and Nye 1977, 23–37; idem 1987, 728–33; Krasner 1983, 356; Lipson 1984, 12–18; Mearsheimer 1990, 11, 44–45; Milner 1991; Morgenthau 1967; Powell 1991; Rosecrance 1981, 705–7; idem 1986, 44–63; Snidal 1990, 704; Stein 1983, 134; idem 1984, 383–86; Viner 1984, 1–29; Waltz 1959, 34–36, 196–98, 219–21; idem 1979, 102–28; Wolfers 1962, 67–115).

4. Interestingly, Arrow's impossibility theorem was at least partly motivated by the problem of whether the Soviet Union could be thought of as having a well-behaved social welfare function (Sen 1985).

5. Ruggie (1986) discusses a transformation in the nature of the actors in the international system, and Kaiser (1990) surveys the changing nature of at least the narrow political causes of war, which seems to reflect changing motivations and preferences.

6. For the reasons I shall discuss, this analysis focuses on Markov strategies, which have this simple functional form, and not on more general strategies in which a state's actions in a given period may be a function of the entire history of allocations leading up to that period and not just of the other state's existing military allocation.

7. More formally, a pair of strategies satisfying this condition constitutes a Markov perfect equilibrium.

8. This formal result parallels some historic experience. The advantage to being the first to mobilize is often taken to be an important source of instability in July 1914. But the states had attempted to offset any advantages to "preemptive attacks on troops disembarking at railheads by concentrating their forces out of reach of such a blow" (Snyder 1984a, 113); that is, the

states, acting strategically, tried to offset the advantages of being the first to mobilize created by innovations in military technology.

9. The alternating sequence of decisions implies $m_1(k) = m_1(k+1)$ for $k = 0, 1, 2, \ldots$ in the summation.

10. Because S_2's allocation in the first period is given exogenously, S_2's payoff will be taken to be

$$\sum_{t=1}^{\infty} \delta_2^{t-1}(r_2 - m_2(t))^{\rho_2}.$$

11. Somewhat more generally, S_1's utility might be taken to be

$$\sum_{t=t_0+1}^{\infty} \delta_1^t d_1^{\rho_1} - l_1,$$

where l_1 represents any additional disutility that being conquered may bring. In a strictly materialistic interpretation of the model in which a state cares only about the resources it allocates to its internal ends, l_1 would be zero. In a less materialistic interpretation in which being conquered means, for example, a lost of autonomy not measured in terms of resources, l_1 would be positive.

12. Many of the simplest formulations of π are not continuous at the origin. Suppose, for example, that the probability that a state will prevail is simply the ratio of its military allocation to the sum of both states' allocations, as long as one of the states has a positive allocation, and zero if neither state has a positive military allocation; that is, the probability that S_1 will prevail is $m_1/(m_1 + m_2)$ if $m_1 > 0$ or $m_2 > 0$ and zero at the origin. Then, π is not continuous at the origin. To allow for formulations like this, π will be permitted to be discontinuous at the origin.

13. See Kim and Morrow 1990 for efforts in this direction.

14. Brito and Intriligator 1985 develop an interesting model in which there is asymmetric information and in which war may lead to many possible outcomes. Their analysis, however, focuses on Nash equilibria that are imperfect.

15. See Fudenberg and Tirole 1991, 501–40, for an extensive discussion of Markov perfect equilibria.

16. For example, the threat to launch a massive nuclear first strike in the presence of secure second-strike forces seems inherently incredible. But such a threat can be part of a Nash equilibrium and is incompatible with the subgame perfection. See my earlier study (Powell 1990, 25–32) for a more detailed discussion of this example.

17. In more complicated models than the one studied here in which information might be imperfect, sequential equilibria or some refinement would be needed to formalize the notion of anarchy. Indeed, even when there is perfect information, as in the present model, one might like to impose further conditions like the requirement that the subgame perfect equilibria also be renegotiation-proof (Farrell and Maskin 1989). As will be evident, the Markov perfect equilibrium I shall characterize is renegotiation-proof if Farrell and Maskin's (1989) definition of a renegotiation-proof equilibrium in the context of infinitely repeated games is extended in the natural way to the present game.

18. Maskin and Tirole offer a more detailed contrast between Markov strategies and the typical supergame punishment strategies in the context of economic models of oligopoly (Maskin and Tirole 1988a, 553; idem 1988b, 592–93; Tirole 1989, 252–54).

19. Downs and Rocke (1990) and Isard (1988) offer recent surveys and contributions to the study of arms races.

20. As already discussed, the game effectively ends if there is ever an attack.

21. Such an $m_1^a(m_2)$ is sure to exist at $m_2 > 0$ because $W_1(m_1, m_2)$ is continuous and S_1's set of feasible military allocations, $[0, r_1]$, is closed and bounded. The assumption that a conflict of interest exists (discussed later) will make the fact that $m_1^a(0)$ may not be well defined irrelevant.

22. This set will be assumed to be nonempty. As will be seen, no peaceful equilibria would exist if this set were empty.

23. To see that $\bar{m}_1(m_2)$ is strictly increasing, let $m_2' > m_2$. Then because $\bar{m}_1(m_2)$ is strictly decreasing in $\overline{W}_1(m_2)$, it will suffice to show $\overline{W}_1(m_2') < \overline{W}_1(m_2)$. By definition, $\overline{W}_1(m_2') = W_1(m_1^a(m_2'), m_2')$. Inspection of W_1 shows that $W_1(m_1, m_2') < W_1(m_1, m_2)$, which implies $W_1(m_1^a(m_2'), m_2') < W_1(m_1^a(m_2'), m_2)$. But $W_1(m_1^a(m_2'), m_2) \leq \overline{W}_1(m_2)$. Putting all of this together leaves $\overline{W}_1(m_2') < \overline{W}_1(m_2)$. Consequently, $\tilde{m}_1(m_2)$ is strictly increasing in m_2.

24. Take $\underline{m}_2$ to be the smallest m_2 in $\mathcal{P}_1$, which is certain to exist because the set is closed. Continuity then implies $\bar{m}_1(\underline{m}_2) = 0$.

25. See the proof of proposition 2 in the Appendix for a formal definition of (m_1^*, m_2^*).

26. It may at first seem that this condition always holds, but this is not the case. At very high military allocations, for example, $\overline{W}_1(m_2^*)$ may be small. In these circumstances, a state may do better to allocate more to its internal ends in the current period and permit the other to attack in the next period.

27. If $(\hat{m}_1, \hat{m}_2)$ is some other point in the intersection of $\mathcal{P}_1$ and $\mathcal{P}_2$ but not in the interior of either of these sets and if the states would prefer to arm if war were inevitable at this point, then it is easy to parallel the proof of proposition 3 to show that a unique Markov perfect equilibrium supporting the allocation $(\hat{m}_1, \hat{m}_2)$ exists and its reaction functions are given by R_1 and R_2 with $\hat{m}_1$ and $\hat{m}_2$ substituted for m_1^* and m_2^*. Points in the intersection of $\mathcal{P}_1$ and $\mathcal{P}_2$ but also in the interior of one of these sets cannot be supported with Markov perfect strategies.

28. To see this, recall $\overline{W}_2(m_1') = P_1(\bar{m}_2(m_1'))$. But $\bar{m}_2$ is increasing m_1, $m_1' \geq m_1^*$, and $m_2^* = \bar{m}_2(m_1^*)$. This leaves $(r_1 - m_1^*)^{\rho_2}/(1 - \delta_2) > \overline{W}_1(m_2')$.

29. A state, say S_1, is essential if it is part of a minimum winning coalition, which, in the context of the Niou and Ordeshook model, means that there exists a group of states that controls less than half of the total resources in the system if S_1 is excluded from this group and at least half if S_1 is included (Niou and Ordeshook 1991, 489).

30. The sufficient conditions identified in proposition 3 may be used to trace out a region of stability, that is, the set of parameter values for which a peaceful equilibrium exists, given a specific specification of π.

31. I am grateful to James Morrow for this point.

32. More formally, π in Ordeshook and Niou's formulation is discontinuous whenever the states allocations are equal. This discontinuity makes the payoff to attacking $W_1(m_1, m_2)$) discontinuous in m_2 and means that $\overline{W}_1(m_2)$ and $\bar{m}_1(m_2)$ may not be well defined.

33. See Milner 1991 for a recent discussion of the role of anarchy in international relations theory and of anarchy's multiple meanings.

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Robert Powell is Associate Professor of Political Science, University of California, Berkeley, Berkeley, CA 94720.

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SOCRATES' ASPASIAN ORATION: THE PLAY OF PHILOSOPHY AND POLITICS IN PLATO'S *MENEXENUS*

STEPHEN G. SALKEVER *Bryn Mawr College*

Plato's Menexenus *is overlooked, perhaps because of the difficulty of gauging its irony. In it, Socrates recites a funeral oration he says he learned from Aspasia, describing events that occurred after the deaths of both Socrates and Pericles' mistress. But the dialogue's ironic complexity is one reason it is a central part of Plato's political philosophy. In both style and substance,* Menexenus *rejects the heroic account of Athenian democracy proposed by Thucydides' Pericles, separating Athenian citizenship from the quest for immortal glory; its picture of the relationship of philosopher to polis illustrates Plato's conception of the true* politikos *in the* Statesman. *In both dialogues, philosophic response to politics is neither direct rule nor apolitical withdrawal.* Menexenus *presents a Socrates who influences politics indirectly, by recasting Athenian history and thus transforming the terms in which its political alternatives are conceived.*

The *Menexenus* is one of the Platonic dialogues least read by students of political theory. But there are good reasons, both thematic and stylistic, for us to consider the dialogue more carefully than we have in the past. This is the dialogue in which Socrates recites to a young and admiring friend named Menexenus a political speech that he says he has learned from Pericles' mistress Aspasia, whom he describes as his, as well as Pericles', instructor in the art of oratory (235e3–7)—a funeral oration (*epitaphios*) for the Athenian war dead.[1] The dialogue can be divided into three parts: (1) an opening conversation between the two men (234a–36d); (2) the *epitaphios* proper (236d–49c), which is subdivided into three parts (a historical account of the people and the *politeia* (regime) of Athens, a narrative of Athenian war deeds from antiquity to the present, and an exhortation to present-day Athenians); and (3) a brief closing exchange between Socrates and his interlocutor (249d–e).

Aspasia, Socrates tells Menexenus, has "glued together" the speech partly, as it seems to him, with "leftover" bits from her composition of the famous funeral oration delivered by Pericles (236b1–6).[2] The *Menexenus*, then, presents itself as Plato's Socratic counterpart to the speech of Thucydides' Pericles. In both speeches we have an account of the deeds of the Athenian past combined with an account of the essential character of the Athenian *politeia*, both seemingly designed to throw light on how Athenians are to understand themselves and to act in the future. But the styles of the two discourses are not similar: Thucydides gives us the speech of a statesman he admires, a speech that could to all appearances have been delivered in Athens;[3] while there can be little doubt that Socrates' Aspasian *epitaphios* and the brief exchanges with Menexenus that precede and follow it make up a comic commentary on the Athenian funeral oration, though in exactly what way and to what end we cannot be sure.

In the *Menexenus*, then, Plato elaborates the contest he establishes in the *Gorgias* between Pericles and Socrates for the right to be called the true Athenian *politikos* (statesman). Moreover, the dialogue suggests the possibility of redeeming what in the *Gorgias* seems unredeemable:[4] the practice of public oratory in democratic Athens. The funeral speech itself in places appears to be an alternative to Pericles' *epitaphios*, rather than comic commentary on it, particularly in its characterization of the Athenian *politeia* as an aristocracy-by-popular-consent (238c7–d2), rather than a democracy, and its advice to both the fathers and the mothers of the war dead to turn their attention inward and concern themselves with their own virtue (247e5–48a7), rather than to fall in love with Athens. Thus, even though the long and detailed military history of Athens from the Persian War to the present that occupies over half of the *epitaphios* in the *Menexenus* (239d1–46a3) is chock-full of errors, omissions, and hyperbole and even though the dialogue's opening and closing conversations are insistently playful and ironic, the speech also presents a picture of good Athenian citizenship that seems plausibly Platonic or Socratic.[5] Socrates' recitation of Aspasia's patchwork *epitaphios* thus opens the possibility that there is an alternative to the rhetoric of flattery and the politics of collective *pleonexia* (unbounded desire) condemned in the *Gorgias*. This is surely a question of the utmost seriousness; yet Plato's treatment of it here is playful and ironic.[6] How can this be so?

Commentators on the *Menexenus* have been sharply divided over the question of whether to read the dialogue as a serious *epitaphios* or as an ironical comment on Athenian politics in general and the institution of funeral orations in particular.[7] Either way, a Platonic dialogue concerning *epitaphioi* must command our attention, since there is good reason to think that the institution of the public funeral oration had a special place in Athenian political oratory, as the occasion on which Athenians gathered not only to hear a eulogy on the war dead but to recall who they were as Athenian citizens. Lincoln's Gettysburg Address is the closest American equivalent—both a eulogy and an exercise in collective self-definition. But Lincoln's speech was an unusual event, while the public funeral oration was a long-standing and customary practice in Athens.[8] According to Nicole Loraux's (1986) detailed study of the *epitaphios*, its function was to provide the occasion on which Athens was invented and reinvented in narrative form. Only a few such orations have come down to us; surely the most famous is Pericles' speech in Thucydides, which is the classic statement of the ethos of Athenian citizenship at the height of its fifth-century flowering. Two others, by Demosthenes and Hyperides, are from later in the fourth century; a speech written by Lysias appeared roughly 10 years earlier than the *Menexenus*, and there is a small fragment from an oration by Gorgias. Even though the number of examples is small, it is clear that the *epitaphios* was a distinct genre,[9] with standard expectations about the ordering of topics and the contents of the message. In the *Menexenus*, Socrates follows the formal order of the *epitaphios*—a prelude identifying the speech as one required by Athenian *nomos* (law or custom), a praise of the dead heroes and of Athens, and a consolatory exhortation (*paramuthia*) to the living—but criticizes the message of other funeral orations and provides an alternative way of characterizing Athenian virtue. Thus, the dialogue appears to contain both a Socratic critique of Athenian self-understanding (along the lines set out in the attack on Pericles and traditional Athenian politics in the *Gorgias*) and an alternative to that self-understanding. Here we have the philosopher speaking not to the city but to *his* city—and in a language and form the city understands considerably better than the language of Socratic dialogue and *elenchos* (refutation). The dialogue gives us the Platonic Socrates acting the role he claims for himself in the *Gorgias*: the one Athenian who truly practices genuine *politikē* (statecraft), the one artful *politikos* in Pericles' city.

But any attempt to ascertain the meaning of the dialogue is radically unsettled by the density and variety of its irony—both Platonic and Socratic. This is, I think, the primary reason why the *Menexenus* has not been treated as a central work in the Platonic corpus. Part of the problem is the difficulty of deciding what it means to say that the dialogue is ironic. Most of those who see the *Menexenus* as ironic, rather than serious, characterize that irony either as a rambunctious parody on the model of Aristophanic comedy, designed to deflate the arrogant pretension of Athenian political speech,[10] or as bitter satire intended to expose the hypocrisy and injustice of the democracy that murdered Socrates and scorned philosophy.[11] But reference to one or the other of these genres—parody or satire—is not sufficient to account for the complex tone shifts of Socrates' playful speech or for the subtlety and essential ambiguity of Plato's written dialogue. Recognizing this, some commentators urge that the core of the dialogue is serious.[12] But the nature of Platonic and Socratic "seriousness" here, as with their "irony", requires more careful consideration than it has so far received. Those who deny that the dialogue is primarily ironic generally understand it to be promoting some particular program of reform for Athenian politics. But there is no clear political program or set of principles to be found in the dialogue—no systematic "theory" in the sense used by contemporary analytic philosophers. In order to make sense of the *Menexenus*, we must first reject the idea that the dialogue has to fall into one of two mutually exclusive genre categories: the comic or the serious.

My alternative to the humorous/serious antithesis is to say that the dialogue is a playful comment on one of the most solemn moments of Athenian politics and that its commentary is not merely negative and dismissive but carries with it some opinions as to how to think about the possibilities and limits of democratic politics. The playfulness of the commentary resides not in its failure to affirm any opinions about politics as true but in its refusal to accept any *formulation* of even the most dearly held opinions as final, as written in stone.[13] The Platonic and Socratic opinions playfully yet seriously asserted by the *Menexenus* involve several aspects of political life; but they always appear to oppose views expressed or implied by Thucydides and by his model of democratic leadership, Pericles.[14] The central instances of this opposition between Plato's politics and Thucydides' are five. First, in what terms should political life be evaluated? For Thucydides and Pericles, the key standards are those of greatness and splendor. Socrates says that the standard for evaluating regimens is set by the example of nurturing nature and by the image of good—not great or noble—human beings (238c). Second, what kind of historical self-understanding is appropriate for democratic Athens? Thucydides' *Peloponnesian War* is a monumental history in which nature and the gods play no part but that of resistances to be overcome[15] and in which Athens is shown creating itself through its daring in wars both defensive and expansionist. Socrates, by contrast, provides a mythic and theocentric story of the origin of the city and follows it with a jumbled, false, and largely chaotic record of recent Athenian wars and battles. Third, what questions should most concern Athenian citizens? Pericles asks his listeners to consider how best to maintain the collective civic project of dynamic Athenian rule. Socrates ignores all collective enterprises and instead emphasizes here, as in the *Apology*, the question of the virtue of each individual and the importance of caring for oneself and for one another. Fourth, what is the appropriate vocabulary of political speech? Thucydides' vocabulary is dominated by terms for greatness, glory, brilliance, and action. Plato in this dialogue, as elsewhere, employs prominently and with particular significations terms like *epimeleia, therapeia, technē, phusis, aretē, dikē*, and *eleutheria* (roughly—caretaking, healing, art, nature, virtue, justice, and freedom). Fifth, what is the appropriate tone in which to consider political matters? Pericles' tone is one of consistent gravity and severity, and Thucydides employs the tragic device of juxtaposing sharply contrasting moments of elevation and wretchedness, as in the abrupt transition from Pericles' *epitaphios* to the account of the plague. Plato's taste runs to a light, playful, frequently shifting tone.

On my reading, then, the *Menexenus* should not be reduced either to parody or satire on the one hand or to a systematic theoretical treatise or a practically plausible Athenian *epitaphios*[16] on the other but should be understood, instead, as a playful reflection designed to provide a starting point for thinking about the kind of public speeches that ought to be made in democratic Athens and polities like it.[17] In trying to assess the meaning of the *Menexenus*, it is crucial not to exaggerate some central Platonic distinctions or tensions (e.g., philosophy/politics, mythos/logos [see *Phaedrus* and *Gorgias*], and serious/playful) by assuming that the weight of the dialogue must fall into one or another of each pair. Hence, it is misleading to ask whether the dialogue is on the side of the polis or of philosophy, serious or playful, expressive of Plato's serious political views or an attempt to subvert Athenian politics. Part of Plato's literary practice is to show us how the philosopher can also be a true *politikos*, how a mythos can be a logos, and how the complex ironies of an artfully written dialogue illustrate serious play. The dialogue can then be seen as a prime example of the kind of political activity indicated by the Eleatic Stranger's depiction of the *politikos* as a master weaver who operates behind the scenes of public life in the *Statesman*: Socrates in *Menexenus* is shown away from the public world of the agora, trying to weave together the strands of gentleness and virility, of the female and foreign (Aspasia) and the male and native (Menexenus).[18]

THE SETTING OF THE DIALOGUE: CHARACTER AND CONCEALMENT

The dramatic date of the dialogue—fixed by reference to events in the Corinthian War down to the Peace of Anatalcidas (245d6–246a1) and hence immediately obvious to Plato's contemporaries—is at the earliest 387 B.C. and thus years after the deaths of both Socrates and Aspasia. (The dating is thus even more anachronistic than the *Gorgias*.[19]) Plato writes a dialogue that in effect identifies itself from the outset as a fiction, as a Platonic invention. The reader cannot believe even in the approximate historical accuracy of the dialogue; this is Socrates made young and fair with a vengeance.[20] As for Socratic irony within the dialogue, the character Socrates presents the logos he recites—his version of that most Athenian of speeches—as having been written by "Aspasia the Milesian" (249d–2), a female foreigner. Nevertheless, Menexenus and the readers of the dialogue cannot be sure whether Socrates may not have invented the story of Aspasia's authorship in order to conceal his own hand in its production. At the end of the dialogue, he tells Menexenus that he will be happy to tell him many other fine Aspasian political logoi so long as Menexenus swears to keep their origin a secret (249e3–5).

The stress on disguise runs even deeper: the longest part of Aspasia's (Socrates', Plato's) *epitaphios*, and the section in which the speech draws conclusions from its narrative history of Athens concerning how present-day Athenians ought to live their lives, is a prosopopoeia of the war dead (something not found in the other funeral orations). The central practical conclusions of the speech are placed in the mouths of the dead heroes. The dialogue thus presents itself as a work of manifold invention and concealment. Plato invents Socrates, who may or may not invent a speech-writing Aspasia, who in turn invents a unison chorus of dead Athenians. Socrates' Aspasia underlines her own artful inventiveness here by saying that she will "say to you now both the things that I heard from them then and those which they would be pleased to speak now if it were possible, basing my conjecture on what they said then. But you must believe that you are hearing from them themselves the message I will give you" (246c4–8). This most political of Platonic dialogues is also perhaps the most complexly ironic; my argument here is that the pairing of political theme and ironic style is no coincidence.

But what does this irony signify? If we rule out, as I think we must, treating the dialogue as either deflating parody or bitter satire, we are left with two elements crucial to Platonic/Socratic irony (crucial in that these are essential aspects of their practice of philosophy): (1) dissimulation or concealment and (2) ambiguity. Irony both conceals the intentions of the speaker and calls attention to the ever-present possibility of linguistic ambiguity. This style suits the practice of Platonic and Socratic philosophical politics in two ways: (1) because the work of the philosophical statesman is not to design institutions, laws, or policies but to persuade citizens of the primacy of the question of the good life or of human *aretē*, over the questions of power, security, and honor; and (2) because the central business of such a statesman is to insist upon the unending importance of asking this question by refusing to give unqualified acceptance to any formulation or doctrine that purports to solve it once and for all. The philosopher's contribution to democratic politics is to place a question on democracy's agenda and, through insisting on the essential ambiguity of language, immunizing democrats against accepting any rule or formulation as final and absolutely binding or correct. There are two principal rhetorical problems to be solved here by the philosophical *politikos*: one is that of finding a way to introduce philosophy into politics without philosophy's undergoing a corrupting transformation into sectarianism;[21] the other is that of giving political advice without calling excessive attention to the philosopher's view that the philosophic life is different from, and better than, the political life.[22] Let us see how the *Menexenus* carries out this work.

THE OPENING DIALOGUE (234A–236D)

The meeting between Socrates and his well-born and politically ambitious young Athenian friend is private. Although we are not told the location of the dialogue, the conversation is initiated by Socrates, who encounters Menexenus as the latter is leaving the agora coming from the Bouleterion, the chamber housing the Council of Five Hundred,[23] where that body is about to select a speaker to deliver the traditional *epitaphios* for the Athenian war dead.[24] The location of the dialogue thus blurs the bright Periclean line between the private and the public worlds. The first words of the dialogue are Socrates' question "*Ex agoras ē pothen Menexenos?,*" "Do you come from the agora or where, Menexenus?" (Socrates being outside the "precinct of public life"),[25] and the answer, "Yes, from the agora, from the Bouleterion, Socrates." We are told by Socrates that the two mean are quite alone and that it is only because they are alone that Socrates is willing to recite the *epitaphios* to Menexenus (236c11–d3). The question why Socrates wants to speak to Menexenus is answered in the exchange between the two immediately following:

> *Socrates.* And why especially were you at the Bouleterion? Or is it clearly because you think you have got to the end of *paideia* (education) and *philosophia,* and thinking you have grasped these things sufficiently, you are ready to turn your mind to greater things, to try your hand at ruling (*archein*) us older folk in spite of your age, o amazing man, so that your family might never fail at providing us with a caretaker (*epimelētēs*)?
>
> *Menexenus.* With your permission and counsel, Socrates, I am eager to rule (*archein*); otherwise, not.

This exchange[26] establishes the dialogue as one between a philosopher and a decent young member of the Athenian political class, one who assumes his future will be occupied with ruling, though only with the consent of others. Menexenus is not a future tyrant. Socrates does not attempt to dissuade Menexenus but, rather, tries to redescribe the political life form its ordinary Athenian designation (*archein* and ruling) to Socrates' characteristic and different way of speaking about political leadership (*epimeleia,* the art of caretaking). Similar redescriptions of ruling or acting as caretaking or something like it form an important aspect of Socrates' hidden political *paideia* in several dialogues. In the *Meno,* for example, Meno's first definition of virtue is that the virtue of a male is "to act (*prattein*) on behalf of the city, acting so as to do well to friends and harm to enemies" (71e1–4). Nearly two pages later, Socrates "reminds" Meno of his definition: "Didn't you say that the virtue of a male is to manage (*diokein*) the city well?" (73a6–7). Like Socrates' concealed redescription of *praxis* (action) as management in the *Meno,* we may expect that his conversation with Menexenus is aimed at transforming the latter's way of thinking about political life from a focus on maintaining a position of leadership in democratic Athens to a consideration of how properly and artfully to care for human beings. This expectation is fully confirmed by the last exhortative section of the *epitaphios* itself, which repeatedly calls on the city to exercise care toward all (e.g., 249c2–3) and on each Athenian citizen to serve and be served in turn (*therapeuein te kai therapeuesthai,* 249c5–6). To the extent that Socrates has gained control over the terms of Athenian political discourse here, the point of the *epitaphios* lies in its transformation of the political world as conceived in speeches from a scene of *praxis* and *archē* (rule) to one of *therapeia* and *epimeleia.*

The very end of the dialogue is a parting exchange in which Socrates playfully insists, and Menexenus accepts, the need to maintain secrecy about this conversation and other similar ones they may have in the future. (Socrates says he will tell Menexenus many other fine political speeches.) In the frame dialogue of the *Menexenus* (236c–d), Socrates presents himself as wanting to hide the *epitaphios* he has heard from Aspasia—at first because of his fear that Aspasia will be angry with him if she discovers he has revealed the speech and a moment later because he fears Menexenus will laugh at him, taking Socrates for an old man still playing like a child. Finally, Socrates says he will gratify Menexenus, since they are alone. His desire to gratify him is, he says, such that he might even strip naked and dance, if that is what it took. This figure of the teasingly reluctant philosopher is familiar from the *Republic.* The depth of the transformation of Athenian political self-conception Socrates aims at is both concealed and underscored by his coy aloofness. The very fact that Socrates has no desire for political power gives his words a power they might otherwise lack.

After learning that the business Menexenus observed at the Bouleterion concerns the selection of a citizen to give an *epitaphios,* Socrates' first response is to remark on how *kalon* (beautiful), in many ways, it is to die in battle, since no matter how worthless you were when alive, you are sure to get a magnificent send-off by speakers who, like wizards, bewitch our souls (235a2). Not only do these orators praise the Athenian dead (worthless though they may have been), but they praise Athens itself in every way, as well as the living citizens and all their ancestors. There is such a riot of patriotic encomium, Socrates says, that for more than three days (235b8–c1), he feels himself to have become bigger, better born, and more beautiful (*meizōn kai gennaioteros kai kalliōn*) than he was before (235b2). Greek foreigners (*xenoi*), who, Socrates says, he almost always has with him on such occasions, are likewise bewitched and view Socrates as more august (*semnōteros*) and Athens as more amazing (*thaumasiōtera*) than before (235b2–8). For as long as the orator's words ring in the ears of the audience, Athenians and Athens are puffed up in their own eyes and in those of the watching world.[27]

"You are always making fun (*prospaizeis*) of the orators, Socrates," Menexenus responds (235c6);

and, of course, Socrates is playing. But the words of Socrates' playful speech not only ridicule the orators but call attention to the way his own auditors respond to Socrates, who can also be charged with wizardry for inducing not a feeling of puffy superiority (*semnōtes*, 235b8)[28] but a sense of bafflement (*aporia*). This effect of Socratic logos is noted not only by menacing foreigners like Meno and local enemies like Anytus[29] but in the description of a very Socratic form of *paideia* described by the Eleatic Stranger in the *Sophist* (230e–31a). At its best, Socratic *elenchos* and *aporia* is designed to get interlocutors to become more gentle to others and more angry with themselves, more determined to inquire about what sort of life they should be leading. There is thus a direct confrontation: the *semnos logos* (august discourse) of the patriotic *epitaphios* as depicted by Socrates removes the problem the aporetic *logos* (baffling discourse) seeks to make pressing. As Pericles says in his *epitaphios* in Thucydides, all that is necessary to achieve virtue, if you are male, is to allow yourself to fall in love with Athens in her imperial greatness and if you are female, to stay at home and out of the way (2.43, 51). But at the same time, the aporetic *logos* refuses to present solutions and so seems to leave its victims, even those who love Socrates, no choice but to repair to the orators—as Cleitophon repairs to Lysias and Thrasymachus (*Cleitophon*, 410c6–8). The introductory dialogue of the *Menexenus* thus sets up this question: Is there any way that the Socratic *politikos* can meet the political orators on their own ground?[30]

Following Socrates in the *Gorgias*, we might put the question this way: Is there a way of speaking to the Athenian demos that unlike the speeches of Pericles, will orient individual members of the demos toward the question of their own virtue, of whether they are sufficiently just and moderate, and not toward the civic projects of "building harbors and dockyards and city walls, and of exacting tribute from subject peoples, and similar nonsense (*phluaria*)" (*Gorgias*, 519a1–4)? The ironic *epitaphios* of the *Menexenus* is a kind of answer to that question.

THE EPITAPHIOS, PART 1: THE GENEALOGY AND CHARACTER OF THE ATHENIAN POLITEIA (236D–39A)

After acknowledging the Athenian *nomos* (law) that establishes the public funeral oration and after noting (except for Socrates)[31] the difficulty of finding words adequate to memorialize deeds of such valor, the funeral orators all open by describing the origin and character of Athens. Emphases differ among the different speakers, but the differences between Socrates and Pericles are especially striking. Unlike other speakers, Pericles spends very little time on the Athenian *genos*, or race (Ziolkowski 1981, 75). What matters most to him about Athens is not the ancient race from which the Athenians among his audience are descended[32] nor even the political freedom that the remote ancestors established but the deeds of the past fifty years that have founded and expanded the Athenian Empire (Thucydides 2.36.2). He does note that the same race of Athenians has lived on the same land of Athens, but this is only in passing and he draws no practical conclusions from it.

Socrates' emphasis in his discussion of the Athenian heritage is in striking contrast to Pericles'. He begins by saying that in praising the war dead, we ought to proceed according to nature (*kata phusin*), just as the dead derived their goodness from nature (237a). Socrates' repeated stress on the importance of nature as a source of goodness (Athenians are unusually *eugeneia* [of good blood] [237a6–7] and that is the first thing we should praise about them) and standard of evaluation stands in sharp opposition to the stress on the extraordinary daring and drive of fifth-century Athenians displayed in Pericles' oration and elsewhere in Thucydides.[33] Socrates' speech dwells on the legend of Athenian autochthony, celebrating the fact that Athenians, unlike other people, both sprung from, and were nurtured by, the land they live on today—Athenians are blessed by sharing both a fatherland and a nourishing mother earth (237c5–d2).[34] Not only are the Athenians autochthonous, but Athens has been unusually beloved of the gods—so much so that while all the rest of the earth was full of wild animals, the Athenian earth alone brought forth only human beings and human food: wheat, barley, olives. Not only that, but the earth, having supplied material nurture for her Athenian children, finds them gods to educate them, "gods whose names it is fitting for us to omit here, for we know them, gods who established our way of life, in our daily regimen educating us in the *technai* first of all, and then concerning guarding the land teaching us how to acquire and use weapons" (238b2–6).

All this ridiculously inflates the specialness of Athens, to be sure, so that no reader could think that Socrates' Aspasian *epitaphios* aims at giving an accurate, or even a believable, history of the origins of Athens. But the speech does more than ridicule patriotic fervor. Socrates' comic genealogy makes the greatness of Athens depend on the good fortune of her natural origin and on divine favor, not on the special bravery of Athenian men of war. While Pericles' civic encomium says nothing about the natural setting of the city of Athens and nothing at all about the gods,[35] Thucydides in his own voice speaks of the causes and advantages of Athenian autochthony in a way that is diametrically opposed to the Socratic account: "Attica, because of the barrenness of its earth, was for the most part free from *stasis* (civil strife) and therefore always inhabited by the same human beings" (1.2.5–6). There was nothing in the natural setting of barren Athens to attract invaders, so that the race of Athenians acquired unity because of the poverty, rather than bounty, of their natural inheritance. For Socrates, this nature represents an inheritance to be treasured; for Thucydides, a weakness to be overcome. The glory of Athens, for Pericles and Thucydides, is in all respects strictly manmade; for Socrates, love of country is expressed by a playful gratitude to the earth and the gods.

Born from the earth and educated by the divine teachers their mother chose for them, Socrates continues, the ancestors of the war dead set up a *politeia,* which should be briefly recalled. Just as Socrates prefaces his story of autochthony with the statement that a good *epitaphios* must praise the dead *kata phusin,* here he begins his discussion of the Athenian regime with a general definition: "A *politeia* nourishes human beings (*politeia gar trophē anthrōpōn esti*); it is noble when it nourishes good humans and the reverse when it nourishes wretches (*kakoi*)" (238c1–2). To show the nobility of a regime, it is necessary to show that it produces good human beings. Since Socrates has already told Menexenus that Athenian funeral orations praise wretches (*phauloi*) along with good men (234c), it would appear difficult to show this for Athens; and Socrates does not even try. Instead, he goes on to characterize the Athenian *politeia* as one that is called by some a democracy "but is in truth an aristocracy with the approval of the many (*met'eudoxias plēthous aristokratia*)" (238d1–2). In Athens, the speech continues, political power is held most of the time by the people (*plēthos*); but at times, the people give power and rule to those citizens who seem to them to be the best. In Athens, unlike other cities, no one is chosen for office because of his wealth or parentage but because he seems wise and good. Socrates never says here that the Athenians do, in fact, choose the wisest and best to rule but only that the regime requires elections for office, which are determined by the opinion of the majority, not by the status of the candidates. The basis for the regime of the best-in-the-opinion-of-the-many is located in the nature of Athens. All other cities are heterogeneous collections of all sorts of people. As a result in those cities, whether they are tyrannies or oligarchies, the citizens regard each other as slaves or masters: "But we regard each other as brothers born of a single mother, and so do not think ourselves worthy to be each other's slave or master, but our natural equality of birth makes it necessary for us to seek conventional legal equality (*hē isogonia hēmas hē kata phusin isonomian anagkazei zētein kata nomon*)" (239a1–2).

None of this recants the often-expressed Socratic view that Athenian democracy generally makes wrong choices about who are its best citizens. In fact, it raises the question of whether those who have seemed wisest and best to the Athenians in the past really have been the wisest and best. Nor is Socrates' contention that the Athenian regime aspires to be an aristocracy, rather than a democracy, incompatible with his critique of Athenian practice elsewhere.[36] Thus, it is misleadingly simple to write off the *Menexenus*' characterization of the Athenian *politeia* as purely pointless humor.[37] Again, comparison with Thucydides indicates a possible context. Pericles, of course, does not call Athens an aristocracy; but he, too, says only that Athens is called by name a democracy, not that it is in truth one. Moreover, he claims that those who rank highest in the city do so not on the basis of wealth but on the basis of *aretē* (2.37.1). His position seems unlike Socrates' only in its lack of frankness in acknowledging that democracy is only a name and in his flattering (not to mention self-serving) endorsement of the city's judgments about the virtue of its choices. Thucydides comments on (and silently endorses) the lack of candor in Pericles' *epitaphios* in his praise of Pericles' leadership. Under Pericles' rule, he says, Athens was at her greatest (2.65.5); and while he ruled, Athens "became in speech, on the one hand, a democracy, but in deed, on the other, a city under the rule of its first man (*egigneto te logō men dēmokratia, ergō de hupo tou prōtou andros archē*)" (2.65.9–10). Plato differs from Thucydides here in two major respects: (1) in setting forward a concept of natural fitness, rather than one of civic greatness, as a starting point for estimating the character and value of a polity[38] and (2) in preferring a style of playful semicandor in public oratory to one of deceptive solemnity.

THE EPITAPHIOS, PARTS 2 AND 3: ATHENIAN MILITARY HISTORY AND ITS LESSONS (239A–49C)

The natural goodness of the Athenian stock having been stated and elaborated, Socrates' Aspasian speech next moves, following tradition, to present an account of how "our forefathers, raised in freedom, have shown all human beings, both in private and in public (*kai idia kai dēmosia*), many and noble deeds, believing that it was necessary to fight for freedom with Greeks against Greeks and against barbarians on behalf of all of Hellas" (239a5–b2). This introduction to the recounting of Athenian military history connects that history with a perceived need to defend the Athenian regime. The Athenians are fighting not for glory but for *isonomia* (equity) and the right to choose the best and wisest among them as their leaders; Socrates also explicitly both recalls and effaces the line between public and private drawn by Pericles. Public deeds are due no special merit in the Socratic/Aspasian account; and in the highly detailed narrative that follows, it becomes easy to see why. After saying that it is unnecessary to recall in bare prose (*psiloi logoi*) the mythic wars against the Amazons, Cadmians, and Argives, since these *erga* (deeds) have already been celebrated in poetry and music, the *epitaphios* goes on to say something about the three major wars of the past one hundred years—the Persian, the Peloponnesian, and the Corinthian—in prose that is very bare indeed.

Various commentators[39] have noticed that Socrates' history involves some systematic distortions. Some of these were not unusual for fourth-century Athenian patriots, such as falsely minimizing the contributions of any other Greek city, especially Sparta, to the Persian War (Thermopylae is not even mentioned); but others harder to explain, such as his almost suggesting that Athens was allied with Sparta against Persia in the Corinthian War, rather than the other way around. There are also striking omissions, one particularly large: Socrates tells the history of fifth-century Athens without mentioning the Athenian Empire at all, let alone celebrating the daring exploits of Athenian adventurers, à la Thucydides. The speech not only relegates Salamis to second place in importance, behind Marathon, among the battles of the Persian War, but treats Salamis, along with Marathon, merely as indications that Greeks could defend themselves against Persians—leaving aside the extraordinary character of the sea battle at Salamis, stressed by Thucydides and by Lysias, namely, that it showed the Athenian polis to be entirely separate from the Athenian earth and the Athenian regime to be located entirely in the spiritedness of Athenian citizens.[40] From Plato's account, one would never know that the Peloponnesian War was in any way related to the fear of Athenian hegemony in Greece. The cause of the unexpectedly terrible character of the war, we are told, was the jealously of the rest of Greece (243b).

Not only does Plato's *epitaphios* deflate the grandeur of Athenian successes, it similarly plays down, in comparison with Thucydides' account, the dramatic despair inherent in the defeat of the Sicilian expedition.[41] Socrates' explanation of the collapse makes it appear a passing inconvenience: "Since our polis was blocked by distance from reinforcing the fleet, bad luck made them give up their plans" (243a3–5). The speech thus manages to convey neither prowar nor antiwar sentiments. Bad luck and low motives conspired to produce undeserved defeats for the Athenians; but just as victories like Marathon and Salamis are treated as wholly instrumental to preserving freedom, rather than as memorable in themselves,[42] so nothing about any of these defeats or sufferings in war seems memorable. Perhaps the clearest thing to be said about Socrates' Aspasian account of the rise and fall of the Athenian Empire is that it is an antitragic one. This can be seen by comparing the *Menexenus* on the defeat at Syracuse with Thucydides' account: "This was the greatest Hellenic deed (*ergon*) that took place during this war, and, in my opinion, the greatest that we have heard of among the Greeks—to the victors the most brilliant (*lamprōtaton*) and to the vanquished the most calamitous."[43] From the perspective of the *Menexenus*, discussion of the goodness of Athens must concern the naturalness of its regime, not the astonishing beyond-good-and-evil greatness of its military exploits. The *stasis* that caused the final Athenian defeat by the Spartans is treated not as a tragic disintegration of civic spirit but as notable primarily for the gentleness and moderation shown by the returning democrats to their oligarchic enemies (something Aristotle remarks on, as well, in the *Constitution of Athens*) owing, the speech says, to their common membership in the autochthonous Athenian *genos* (244a1–3). Once the *stasis* was concluded, Athens, for what we cannot doubt are excellent reasons, resolves never again to fight in the cause of the freedom of Hellenic *xenoi*, either against other Greeks or against barbarians. Unfortunately, Sparta decides to take this opportunity to enslave other Greeks.

The account of the ensuing Corinthian War (244d–46a) is similarly confused and prosaic. No pattern or ordered meaning arises here any more than from the account of the other greater wars. Everyone begs for Athens's help; and, being, as always, too compassionate (244e3), Athens is once again at war—with Sparta and in aid of Persia in spite of the fact that the Athenians, as unmixed Greeks, hate barbarians more than any other Greek does (245c6–d2). Nonetheless, it is Athenians who free the Persian king and drive the Spartans from the seas (245e–46a), bringing us to peace at last and to this *epitaphios*. Socrates' summing up of the deeds of Athenian men of war is as equivocal as the formless narrative itself: "Now many and noble deeds of the men lying here and of others who died for the city have been told, yet many more and nobler are those which have not been mentioned; for many days and nights would not be enough if one wanted to relate all of them" (246a5–b2). If the speech ended on this note, we would surely say that Socrates has managed to find an *epitaphios logos* guaranteed not to leave the audience feeling large and *semnos* for an instant, let alone three days.

The particular successes and failures of Athenian military endeavor, shorn of greatness and brilliance, provide no point of reference for future action. The ability to make war is necessary if a city is to retain its freedom, and this calls for courage in the face of danger and difficulty; but the deeds of war have nothing of Thucydidean greatness about them, and their chronicle is thus appropriately boring and repetitious. This difference can be seen by placing side by side the moral lessons drawn from the *erga* in the Periclean account and that of the *Menexenus*:

> By giving their bodies for the common good, these men win for themselves praise that never ages, and the most significant (*episēmotaton*) of sepulchres, not the one they lie in here, but that one in the opinion of men where they leave behind their everlasting memory, always there to inspire both word and deed at the critical moment. (Thucydides 2.43.2)
>
> Therefore it is necessary that every man, remembering these men, exhort their children, just as in wartime, not to leave the rank (*taxis*) of their ancestors, nor to yield to wretchedness (*kakon*) and make a retreat. And I myself, o children of these good men, I both exhort you now and will remind and call on you whenever I meet any of you in the future to be eager to be the best you can be. (*Menexenus* 246b2–c2)

The immortality stressed in Thucydides drops out in Plato; and the lesson taught by the deeds of the war dead shifts from a standing inspiration to future heroism to a reminder of the importance of keeping your place in the hoplite phalanx and of trying at all times—not just in the pressure-filled *kairos* (critical moment)—to live the best life you can.

But the speech does not end here (as several other *epitaphioi* do) but goes on not only to console the audience but to advise them on how to live their lives. Here, Plato's aspiration to match and exceed the scope of Pericles' oration is evident. J. E. Ziolkowski, in his survey of the funeral orations, notes that "the exhortation to the audience in Thucydides and Plato is not only longer, but also of a different nature from that of the other funeral speakers. The significant difference is that the other speakers include no advice for future action as Thucydides and Plato do. . . . Even between the latter two there is an important difference: Thucydides gives directions for personal conduct in the present war; Plato gives general advice for conduct in war and peace" (1981, 159–60). The tone of the *epitaphios* shifts markedly at 246b2,[44] as the speech leaves behind the business of recounting the zigzag fates of war and turns to the task of reminding and exhorting the audience to be eager to excel in virtue. It is at this point that the concealment of the speaker's voice is extended yet one more stage, and Aspasia asks us to hear her voice as if it were the combined voice of the dead heroes. The prosopopoeia, unlike Pericles' speech, does not call on the survivors to try to match the virtue of the dead by an all-embracing erotic commitment to Athens; nor does it hold out to them the promise of immortal life as part of the undying memory of Athenian greatness. Instead, the living are urged to do better than the dead and to do so in a truly extraordinary but thoroughly Platonic way—by interpreting the Delphic motto *Mēden agan* (nothing in excess) to mean that one's virtue depends wholly on oneself and that one ought to treat life and death lightly and moderately:

> The ancient saying *Mēden agan* seems nobly said, and it truly is well said. For that man who makes everything that concerns his *eudaimonia* (happiness), or most of it, rest on himself and not on other human beings (so that his own good and bad do not perforce wander up and down with theirs), that man is the one best prepared to live; and it is he who is *sophron* (moderate) and manly and wise, it is he who while gaining or losing wealth or children will be especially persuaded by the proverb, because he neither rejoices nor mourns overmuch, since he trusts himself. . . .We ask both fathers and mothers to live the remainder of their lives holding to this same thought (*dianoia*), and to know that it is not by mourning and lamenting us that they will especially please us. . . . They would gratify us most by bearing [their sorrows] lightly (*kouphōs*) and with measure (*metriōs*). (247e5–48c2)

The epitaphios concludes with Aspasia resuming her own voice, asking children to listen to their dead fathers, and assuring parents of dead sons that the city will nourish their old age and care for them, repeating again the phrase "both in private and in public" (*kai idia kai dēmosia*, 248e4), previously used to describe the noble deeds of Athenians (239b1). She ends by reminding her audience of the political centrality of *epimeleia* and *therapeia* (the polis appearing here in the image of Nurse, rather than, as with Pericles, the magically compelling Beloved). Socrates draws the curtain on this make-believe epitaphios by reminding his audience that he has been listening to a female foreigner: "That is the logos, o Menexenus, of Aspasia the Milesian" (249d1–2). Promising not to break confidence, Menexenus asks Socrates to tell him more. Promising that we have not heard the last word, Plato has his Socrates end the drama by assuring us, "Well, these things will be done."

CONCLUSION: EARTHLY IMMORTALITY AND THE GOOD LIFE

The playfulness of the written Platonic dialogue makes it an inappropriate vehicle for setting out policy proposals or even unambiguous and systematic theories of government. But this does not mean that Plato's intervention in the political life of Athens is without point. In the dialogue, Plato sets in motion a way of talking about political life that both recalls and challenges Thucydidean political discourse, giving political life a different aim and different problems to solve. Perhaps the clearest way of expressing the challenge to Periclean morality asserted by the *Menexenus* is to say that it tries to direct attention away from earthly immortality and political greatness and toward a concern with living the life we have as well as possible, asking us to take our bearings in politics from our nature, rather than from a narrative of remarkable events. Part of this challenge calls for a revaluation of the Periclean distinction between public and private life, between the brilliantly noble and the merely good. The *Menexenus* may thus be said to carry on a project begun in the *Gorgias*. In that dialogue, Socrates comes closest to touching Callicles in a long speech in which he advises him not to be concerned with oratorical success or with not dying: "For anyone who is truly a man will ignore the length of time he will live and not set his soul's desire on that; but leaving all that to the god and trusting the women, who say that no one can flee his destiny, he must inquire about this thing: In what way will he best live whatever life he has?" (512d8–e5). Our worries about personal security are to be shifted not to an identification with sublime public greatness but toward inquiry concerning the good for each of us.

But why does Socrates connect that key admonition with the apparently superfluous remark that the real *anēr* (man) will trust women and forget about immortality? Here we may glimpse the significance of Aspasia in Plato's political philosophy. The gendering of the political voice that makes the *Menexenus* such an unsettling dialogue from the outset (Why does Socrates say the *epitaphios* is Aspasia's and not admit to its authorship himself, as Menexenus, joined by generations of commentators, so clearly wants him to do?) derives from Plato's interest in bringing to light questions and issues that Pericles and Thucydides relegate to the silence of the private life and in calling our attention to the fact that this is precisely what he is doing. Plato's Aspasia is no more a woman or less a fiction than his Diotima[45] (or, for that matter, his Socrates); but the mask of gender, and the *aporia* such gendering may produce, is central to the ironical rhetoric of Plato's philosophical politics. That rhetoric is designed not directly to replace how politicians speak about the world but to incline them toward self-critical reflection about that way of speaking. Socrates' place, like that of the true *politikos* described in the *Statesman,* is not at the front of the assembly or the Bouleterion but by the edge of the agora, weaving sense into the language of political deliberation.

Notes

A version of this paper was presented at the 1991 annual meeting of the Southern Political Science Association, Tampa, Florida. For helpful comments, I thank panel organizer David Schaefer and fellow panelists Patrick Powers and Michael Zuckert.

1. Menexenus is the agreeable and playfully eristic young companion of Lysis in the *Lysis* and is said to have been present at Socrates' death in the *Phaedo.*

2. I will assume that Socrates refers here to the famous oration recorded in Thucydides' *Peloponnesian War* (2.35–46). This is the opinion of most scholars, though some have thought it might refer to some other Periclean oration. Several important parallels between the two speeches are good evidence for seeing the speech in the *Menexenus* as a response to the one in Thucydides. Kahn (1963) makes a strong case for this position. According to Socrates, the part of the *epitaphios* he is about to recite that was not glued together was improvised by Aspasia on the spot, with an eye to what had to be said. There are parallels here with Thucydides' account of how he composed the speeches he presents in the *Peloponnesian War,* that is, partly from previously written sources and partly on the basis of what he thought appropriate (1.22).

3. Thucydides says that his speeches aim at capturing what must have been said on each occasion (1.22).

4. In the *Gorgias* Socrates, calling himself the true *politikos,* says that he could never make a persuasive speech in front of an Athenian jury (521d–22a, c).

5. The exhortation (*paramuthia*) to attend to the excellence of one's own way of life is similar to the prodding of Socrates the gadfly in the *Apology.* As for the reference to Athens as a democratic aristocracy, recall that even in *Republic* 8, the democracy is said to be the one regime that contains all species of regimes (557d4). Moreover, the *Republic,* which for Vlastos expresses "Plato's normal view of the credo of democracy in this middle period of his life" (1973, 192), defines democracy as that regime in which the citizens share power equally and in which public offices are filled for the most part by lot" (8.557a2–5). But this implies that to the extent that offices are filled by election, rather than lot, as is the office of public funeral orator, the regime is not, strictly speaking, a democracy but moves in the direction of aristocracy. While a demos or *plēthos* can never become philosophic (6.494a4), it is not impossible to exhort (*paramuthoumenos*) a *plēthos* to be open to philosophy, since it, unlike the *oligoi* (the few), is by nature gentle (*praon*), rather than harsh (6.499d10–500a7)—even if Adeimantus does not quite think so. In the *Laws,* the Athenian Stranger argues that it is easier to bring about the best regime from some forms of democracy than from an oligarchy (4.710e3–6). And in the *Statesman,* the Eleatic Stranger says that living in a lawless democracy is better than living in a lawless oligarchy or monarchy (303b). As Socrates says to his old friend in the *Crito,* the many are capable neither of great goods or great evils (44d).

6. Dodds calls the *Menexenus* a satyr play appended to the end of the tragic *Gorgias* (1959, 23–24).

7. For a survey of the controversy from antiquity to the present, see Clavaud 1980, 15–77. The ancient commentators tended to regard the dialogue as a serious *epitaphios.* Cicero, for example, says that it was recited every year at Athens (*Orator* 151). Since the eighteenth century, modern scholarly opinion has been largely on the side of a comic reading. There have, as with other perplexing dialogues, been claims that the dialogue must be spurious; but the ancient testimonies (including references to the dialogue in Aristotle's *Rhetoric*) make this way of solving the problem of reading the dialogue less attractive than usual. The most common way of handling the dialogue is to classify it as genuine but trivial.

8. Thucydides 2.24. See also Isocrates, *Panegyricus* 74, written about 380 B.C.

9. J. E. Ziolkowski (1981) provides a most useful catalogue of genre characteristics, along with some suggestive consideration of individual differences among the speeches.

10. Nicole Loraux's 1974) is the best version of this view. She reads the dialogue as a countercharm designed to perform a katharsis on those whose self-image has been charmed into passive narcissism by uncritical paeans to Athens like Pericles' *epitaphios.* The difficulty with treating the dialogue as sheer parody is evident in the following characterization by Gregory Vlastos: "And just in case we have forgotten how foreign to Socrates' nature is this kind of performance, almost his last words before beginning the speech are that he will be 'playing' (236c) and 'dancing' (236d)" (1973, 190). But this is absurdly to assume that Socrates never characterizes his own seemingly serious speech as playful. (For an important example relating to the "seriousness" of his critique of democracy, see *Republic* 7.536c1–4.) It is also to forget or ignore Socrates' statement in *Phaedrus* that all written logoi are a kind of playing (275c).

11. Coventry's (1989) version of this point of view is worth reading, as is Henderson 1975. For a less persuasive and more extreme version, see Stern 1974. But Coventry assumes—wrongly on my view—that Plato's political goal is to found a city like Kallipolis and that *Menexenus* is a bitter response to the failure of Athens to adopt anything like that goal.

12. For Kahn (1963), the dialogue is an attempt to rebut Periclean imperialism and reorient Athenian public opinion in the direction of Panhellenism (a position not unlike Isocrates') by recalling the old days of Greeks united against Persians at Marathon. For Loraux (1974), *Menexenus* is a countercharm aimed at breaking the spell-binding powers of Periclean rhetoric and freeing Athenians from the passive narcissism such rhetoric induces. For Clavaud (1980), the dialogue is a more generalized attack on the excesses of contemporary Athenian rhetoric, particularly as influenced by the teaching of Antiphon.

13. As for ambiguity, the speech leaves us with no clear precepts about the duties of citizenship and seems to baffle any attempt to be certain that Socrates' words are unequivocal, that they mean exactly what they say. But this feature is common to the whole Platonic–Socratic enterprise. For a strong and comprehensive argument for the essential and intentional ambiguity of the language of the dialogues, see Desjardins 1990. See the *Phaedrus* 276d–e on the essential playfulness of good written speeches.

14. I share Peter Euben's view that Thucydides "extends and elaborates Periclean foresight and that his theory embodies the virtues of Periclean leadership. In this sense Thucydides' political theory is modeled on Pericles' words and wisdom, though the latter cannot simply be a model for it since one is a speaker in the assembly whereas the other is a writer on events that took place there" (1990, 191). Thus, Pericles is constrained to call Athens a democracy, while Thucydides can say that it was a democracy only in word, while in deed it was, while Pericles was general, ruled by its stalwart first *anēr* (man), not by the wavering demos (2.65.9–10). Nothing matters more to either Pericles or Thucydides than civic greatness, and it was under Pericles that Athens was at her greatest (2.65.5).

15. For instances of nature or human nature understood as a problem, rather than a solution, in Thucydides, see 3.20 (Cleon), 3.45 (Diodotus), 3.82 and 85 (Thucydides on *stasis* [stagnation] at Corcyra), and 5.105 (the Athenians at Melos).

16. Whereas Isocrates' *Panegyricus* or Lysias' *Epitaphios* for example, could be so considered. It is similarly misleading to treat the *Menexenus* as, in effect, a "political pamphlet," as Kahn (1963) does. Plato's intentions and style are less straightforward than those of the *epitaphiast* or pamphleteer, though Kahn is surely right to assert the importance of placing the dialogue in the context of political debate at Athens in the early fourth century.

17. See *Phaedrus* 276d1–8 on the noble playfulness of the written speeches that the person who knows about the just, the good, and the beautiful sets down "as reminders for himself when he comes to the forgetfulness of old age, and for all others who follow the same path [or track, *ichnos*] with him." Plato calls into question a number of central dichotomies: seriousness and play, philosophy and politics, logos and mythos.

18. In terms of another of Socrates' self-characterizations, the *Menexenus* might be seen as a demonstration of his practice of the matchmaking *technē* (*Theaetetus* 149d–50a).

19. On the confusion surrounding the dramatic date of the *Gorgias*, see Dodds 1959, 17–18.

20. The situation here is thus quite different from that in the *Crito*, the one dialogue in which the Platonic Socrates presents an Athenian political speech (that of the personified Laws of Athens) of comparable breadth.

21. This formulation is taken from Leo Strauss (1968, 209–10). In his debate with Alexandre Kojève, Strauss says that as soon as the philosopher becomes more committed to a solution than to the continual awareness and restatement of the fundamental problems, he ceases to be a philosopher and becomes a sectarian, something Socrates never did. Strauss's distinction between philosophers and sectarians applies with special force to the philosophical politics of the *Menexenus*.

22. In the *Philebus*, there is an intriguing exchange between Philebus, Socrates, and young Protarchus, concerning the dispute between the two older men as to whether the life of nous, or mind, is preferable to the life of pleasure. Responding to Socrates' praise of nous, Philebus says, "You puff up (*semnuneis*) your own god, Socrates" (28b1). After a few lines, Socrates asks Protarchus if he has disturbed him by "playfully puffing up (*semmunōn en tō paidzen*) nous" (28c2). Protarchus says that his is at a loss; and Socrates responds that "all the wise agree—thus puffing themselves up (*heautos ontōs semnunontes*)—that *nous* is our king over heaven and earth. And perhaps they speak well" (28c6–8). The importance of the verb *semnoō* for Socrates' critique of *epitaphioi* will be apparent in n. 28. Stanley Rosen calls attention to the importance of this passage (1991, 161). But Rosen radicalizes and distorts the passage by ignoring both Socrates' reference to play and the nasty edge Socrates gives to *semnoō* (which Rosen translates as "exalt").

23. This is the way Philip Manville characterizes the agora (1990, 194). Notice, however, that Socrates is very far from the otherworldly philosopher whom Socrates describes in the Theaetetus, who does not even know the way to the agora.

24. On the council, see Ober 1989, 138–41.

25. Thucydides remarks on the long-standing custom of the public *epitaphios* while introducing Pericles' speech (2.34), as does Isocrates (*Panegyricus* 74); and all the surviving speeches, including Socrates' and Pericles', open by acknowledging its traditional character.

26. Coventry calls attention to the importance of this passage (1989, 1–2), as does Méridier in his Budé edition of the dialogue, where he aptly comments on it as follows: "In a strict sense, the epimeletes were distinct from ordinary magistrates (*archontes*) in that they acted only on instructions given by the people (for example, ambassadors, customs officials, public works commissioners). But here, as often with Plato, the word is taken in a general sense" (1931, vol. 5, pt. 1, 83–84n).

27. Compare the bewitching effect of the speech of the Laws of Athens on Socrates at the end of the *Crito* (54d2–7). Both are logoi that put an effective stop to the possibility of any future logoi. This is emphatically not the case with the ironic *epitaphios* of the *Menexenus*, which not only permits, but demands, further commentary and interpretation.

28. *Semnos* and *semnotēs* can have a positive meaning, suggesting "holy, august, reversed, majestic." Context makes it likely that Socrates does not intend this, but we can never be sure. Socrates uses a related word, the verb *semnoō*, in the *Gorgias* when describing a truly useful *technē*, that of the ship's pilot. Unlike the useless orator, the pilot does not puff himself up (*ou semnunetai*, *Gorgias*, 511d4)

29. *Meno* 80a–b, 94e. Just as Socrates charges in the *Menexenus* that *epitaphiasts bewitch* their audience into a helplessly *semnos* condition, so Meno uses the same verb (*goēteuein*) in charging Socrates with bewitching his interlocutors into helpless *aporia* (80a1–4). This is what leads Loraux (1974) to see the *Menexenus* as a Platonic countercharm against the patriotic drug of orations like Pericles'.

30. That the *politikos* could meet the orators on their own ground seems to be impossible, at least in a law court, according to the true *politikos* of the *Gorgias* 521d–22a.

31. Ziolkowski 1981, 65. Socrates has remarked to Menexenus that nothing could be easier than finding ways of praising Athens and Athenians before an Athenian audience (236a).

32. Pericles says that his speech can be heard with profit by both Athenians and *xenoi* (Thucydides 2.36.4).

33. See, e.g., the speech of the Athenians at the first Spartan Congress (ibid. 1.74).

34. Autochthony and nourishment by the motherland is also mentioned (though with much less emphasis) later in the fourth century by Demosthenes (*Epitaphios* 5).

35. Stories about the gods and their intervention in human affairs are, for Thucydides, relics of the outdated Homeric tradition of Greek history he hopes to supersede. Socrates' attitude toward divine mythology, as he explains it to young Phaedrus, is that since he has to spend all his time trying to know himself, he has no leisure left for working out secular explanations for events popular belief credits to the gods and so is perfectly willing to leave popular mythology alone (*Phaedrus* 229e–30a).

36. See the citations collected in n. 5. Recall particularly that democracy for Plato is a generic type that admits of a greater variety of instantiations than either tyranny or oligarchy and for the Athenian Stranger in the *Laws*, the best regime can more easily be achieved in transition from a relatively good democracy than from any sort of oligarchy. According to the definitions of democracy and oligarchy in Aristotle's *Politics* 3, of course, the institution of election by vote, so central a feature of political life in fifth and fourth century Athens, is an aristocratic characteristic; pure democrats choose their leaders by lot, oligarchs by wealth.

37. It is likewise misleading to write it off as simple parody, as in Vlastos's characterization of "the primped-up Athens of 'Aspasia's' speech" (1973, 195).

38. Thucydides' references to nature and to human nature almost always suggest anticivic tendencies, rather than any possible basis for *nomos*. For example, according to Cleon, human beings are naturally inclined to feel contempt toward those who serve them, while for Diodotus, human beings individually and in cities are naturally inclined to injustice (3.39.45). The Athenians at Melos say that by nature, both gods and humans rule wherever they can (5.105). Thucydides comments on the wretchedness of unchecked human inclinations in his own voice in his discussion of the *stasis* at Corcyra, and later in the same discussion says that human *phusis* aims at overturning human *nomos* (3.82.84). The only reference to nature as providing an appropriate standard for evaluating conduct is in regard to women. For example, Pericles, at the end of his *epitaphios*, says that women should follow their nature and stay out of public life (2.45.2); and Thucydides tells of women taking an active part against their nature (*para phusin*) in the fighting at Corcyra (3.74.1–2). Unlike women, real men for Thucydides must aim at transcending and controlling nature. Another roughly contemporary instance of *phusis* used as a standard only for women is to be found in the telling of the story of the war with the Amazons in Lysias (*Epitaphios* 4).

39. My discussion here relies on Kahn 1963 and, esp, Henderson 1975.

40. Compare Plato's brief mention of the battle of Salamis with the ecstatic account given by the Athenians at the first Spartan Congress in Thucydides 1.74, and with Lysias, *Epitaphios* 32–46.

41. Henderson notes this (1975, 42).

42. Compare Lysias: "By means of many toils, shining contests, and most beautiful hazards, they made Greece, on the one hand, free, and their own fatherland, on the other, surpassingly great". (*Epitaphios* 55).

43. Thucydides 7.87.

44. This is noted by both Henderson (1975) and Coventry (1989).

45. On the reason for inventing a woman as Socrates' instructor in erotics (the one art he calls his own), see Halperin 1990. Aspasia's role as Socrates' instructor in the art of political oratory reinforces, I believe, Halperin's commentary on Diotima.

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Stephen G. Salkever is Professor of Political Science, Bryn Mawr College, Bryn Mawr, PA 19010–2899.

RESEARCH NOTES

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CONTEXTUAL DETERMINANTS OF FEMINIST ATTITUDES: NATIONAL AND SUBNATIONAL INFLUENCES IN WESTERN EUROPE

LEE ANN BANASZAK and ERIC PLUTZER *Iowa State University*

We augment the survey-based studies of attitudes toward feminism with comparative, contextual perspectives emphasizing the importance of social structure and culture. In doing so, we are able to assess the relative merits of two very different structural theories. Most researchers implicitly assume a simple linear effect of social context on attitudes. On the other hand, some early works on American conservatism suggest to us that status discontent may be a better explanation. We explicate these two approaches and derive a series of testable hypotheses for each. We then examine the validity of these theories utilizing data from nine European nations.

As gender politics became increasingly important in the politics of the Western democracies, researchers have become interested in understanding why some individuals endorse, and others oppose, the efforts of the women's movement to enhance the legal and economic status of women. By far, the largest number of studies have examined attitudes within particular nations, focusing on individual-level traits, such as education, marital status, employment, and religion (Andersen and Cook 1985; Conover and Gray 1983; Davis and Robinson 1991; Klein 1984; Plutzer 1991; Tedin et al. 1977; Wilcox 1991). They find, typically, that men and women in the United States and Western Europe are more likely to endorse feminist goals, support the women's movement, or show high levels of feminist consciousness if they are highly educated, have little religious involvement, and live in families where women work in the paid labor force.

On the other hand, some authors have conducted historical and comparative studies of nations that attempt to explain aggregate levels of profeminist sentiment by referring to various aspects of a nation's social structure or institutions. For example, comparative studies of the emergence of women's movements (generally case studies or comparisons among two or three nations) have emphasized the importance of structural features of Western societies and labor markets (e.g., Chafetz and Dworkin 1986; Dahlerup 1986). Norris (1984) and Klein (1987) provide the most extensive of these comparative studies; they compare public opinion toward feminism in the 10 member nations of the European Economic Communities. Unfortunately, analysis of 10 or fewer nations does not afford the possibility of weighing several theoretical explanations against one another; in statistical terms these models are underidentified for lack of a sufficient number of data points or degrees of freedom.

In addition, the cross-national studies mask a large amount of internal variation within countries. To give just one example, Norris (1987) ranks France as one of the most favorable nations in terms of female participation in the labor force. Yet a close examination shows some regions in France with female labor participation rates lower than the lowest-ranked nations. To illustrate, the Eurostat (1984) *Yearbook of Regional Statistics* reports that the percentage of women employed full-time varied greatly within each nation. In France, female employment rates ranged from 15% in Corsica, to 31% in Alsace, to 43% in the region encompassing Paris. Thus, national social indicators provide only a poor approximation of the immediate impact of social structure on individuals.

Yet, the cross-national findings compel us to reexamine the individual-level survey results. They clearly suggest that several key aspects of social structure may be crucial to attitude formation. In particular, in nations where women's participation in the economy is high, support for feminist goals is also high. In addition, the literature also suggests that low religiosity, ease of divorce, and other characteristics of modernity are associated with profeminist sentiment. The implication of structural theories such as these is that in principle, cross-national variation in support of feminism can be explained by the effects of several aspects of social structure and culture.

We take an intermediate approach that augments the survey-based method with comparative contextual data on social structure and culture. This allows us to supplement individual-level explanations with structural theories of attitude formation. In so doing, we are able to assess the relative merits of two very different structural theories. On the one hand, theories of social context suggest that individuals are influenced by their environment, producing a simple linear effect of social context on attitudes. On the other hand, some early works on American conservatism suggest to us that status discontent may be a better explanation (Gusfield 1963; Hofstadter 1963). Theories of status discontent suggest that individuals react against the social context especially when the context is viewed as hostile to their own status. We shall explicate these two approaches and derive a series of testable hypotheses for each, then examine the validity of these theories utilizing data from nine European nations.

SOCIAL STRUCTURE AND POLITICAL SOCIALIZATION

The basic tenet of the contextual approach is that the social composition of society has important consequences for political attitudes and behavior. Tingsten's study of 56 Stockholm election districts provides a classic example (Tingsten 1963): he found that working-class voters were especially likely to support Socialist parties when they constituted a majority of their election district. Huckfeldt (1986) reviews this and other contextual studies, all of which point to the fact that the social composition of one's neighborhood or region has a direct effect on political behavior and election outcomes.

Klein (1987) advances an analogous position, arguing that changes in the political economy in the West required the increased participation of women in the paid work force.[1] As the sexual division of labor changed, there were corresponding changes in family structure (e.g., smaller families) and family roles. The end result, Klein argues, was a "new definition of womanhood" which was necessary, although not sufficient, for the rise of feminist consciousness and activism. Thus, societal characteristics are a critical component of political socialization.

Individuals' support of gender equality, Klein states, is partly related to "exposure to nontraditional experiences" (1987, 35). An individual's exposure comes not only from contextual effects but from personal experiences (individual-level effects), as well; and there is now substantial evidence of such effects at the individual level. Studies show, for example, that female employment experience is associated with profeminist attitudes among women (Banaszak and Leighley 1991; Davis and Robinson 1991; Klein 1984; Wilcox 1991), their husbands (Davis and Robinson 1991; Klein 1984; Plutzer 1991; Smith 1985) and their children (Powell and Steelman 1982).

It is theoretically possible that observed cross-national differences are caused entirely by varying numbers of citizens in each nation having *personal* exposure to nontraditional experiences. Yet the empirical literature rules this out. We have elsewhere shown that substantial cross-national differences remain after statistically controlling for a comprehensive set of individual-level predictors (Banaszak and Plutzer n.d.).

At present, then, we cannot account for cross-national differences in profeminist support with individual-level effects alone. In addition, we have a growing literature suggesting that exposure to a society that is structurally and culturally nontraditional fosters profeminist beliefs. Thus, we will examine the applicability of contextual theories to the formation of feminist values and the stronger argument that contextual variables can account for all observed cross-national differences in feminism.

SOCIAL STRUCTURE AND STATUS DISCONTENT

A rather different causal process is suggested by theories of status politics. Two early proponents of theories of status politics, Hofstadter (1963) and Gusfield (1963) explain several strands of right-wing politics in the United States as emerging from groups that experience a loss of prestige or status in the society. In essence, it is a theory about traditional groups that lose status as the society around them becomes more modern; their reaction is retrenchment and support of extremely conservative positions. With respect to women's political sentiments and behaviors, Duverger claims that housewives will cling more strongly to traditional views in areas where many women have acquired nontraditional life-styles (1955, 128).

According to this literature, then, status discontent should be restricted to those with traditional lifestyles in the least traditional settings. We are therefore led to predict that antifeminism will be high among traditional married individuals but especially high when they live in areas with high divorce rates. We should expect full-time homemakers to be especially conservative in areas where large proportions of women work, and so on.

However, it is possible that status discontent operates in two directions. Although virtually all previous theoretical treatments focus on conservative outcomes, we have elsewhere suggested a parallel argument that predicts leftward shifts (Banaszak and Plutzer n.d.). For those exposed to nontraditional experiences such as divorce, a university education, or female labor force participation, the effect on feminist consciousness could be enhanced in traditional settings where such life-styles constitute a small minority. Conversely, this approach suggests that feminist sympathies may be suppressed in areas where women's gains are already substantial (perhaps due to complacency). This approach has not been tested previously, and we shall formally evaluate it here.

DIMENSIONS OF TRADITIONALISM AND WOMEN'S STATUS

We must ask, then, what specific structural variables affect people's political socialization or status discontent and might explain national differences in profeminist attitudes? The theoretical literature on this point is rather thin; but the various works point to the same key variables: women's educational attainment and opportunities, women's labor force participation, role options beyond traditional wife and homemaker, and religiosity/secularization.

Education. A number of authors point to women's educational opportunities as crucial in the evolution of feminism. Chafe (1977) notes that early waves of

U.S. feminism coincided with the growth of women's colleges. Lipman-Blumen (1984) argues that education is not only an important resource for women but inhibits traditional gender myths from being accepted by men.

In addition, Guttentag and Secord (1983, 32) argue that high levels of female achievement in higher education create a pool of women who can act as a bulwark against institutions that advance traditional and sexist norms. Huber and Spitze (1983), however, argue that education functions primarily as an antecedent variable to women's labor force participation. If Huber and Spitze are correct, educational attainment should have no net impact after statistically controlling for the labor force participation.

Women's Labor Force Participation. Several authors suggest that women's labor force participation also influences gender role attitudes, support for feminism, and participation in the women's movement. (Chafetz and Dworkin 1986; Dahlerup 1986; Gerson 1987; Klein 1987; Lipman-Blumen 1984; Norris 1987). Labor force participation initiates these changes in several ways. First, women's labor force participation increases women's economic resources, dispels myths about women's inabilities to participate, and increases their power in family life. In addition, as more women enter the labor force, the society as a whole becomes infused with stories of discrimination leading to acknowledgements of existing inequalities. Finally, large numbers of women in the labor force create new social networks that permit the sharing of experiences among women, the expansion of women's networks, and the development of gender consciousness.

Divorce Rates. Klein (1987) suggests that divorce rates indicate movement from traditional to modern family life. Plutzer (1988) argues that the experience of divorce itself, with women having increased reliance on their own earnings and having to play multiple parental and familial roles, fosters profeminist sentiment directly. At the aggregate level, a large number of women experiencing divorce effects the number of people confronting gender-based obstacles. Although divorce tends to hurt women economically and help men (Weitzman 1985), the option for women to leave an unsatisfying or violent marriage is generally viewed as an indicator of freedom from rigid patriarchal family norms.

Religiosity. Himmelstein (1986) and Mayer and Smith (1985) point to religion as a system-level factor having implications for politics and gender attitudes. With few exceptions, Western religious institutions have advanced and reenforced traditional gender norms that include separate and subordinate positions for women. These have been used by lay members to justify unequal social arrangements, as well. Thus, the process of secularization necessarily weakens an important antifeminist force and should promote profeminist view in the society.

We should note that these four dimensions are not indicators of a common factor. In the preceding paragraphs, we have suggested that each is related to feminism by a distinctive causal mechanism. Moreover, at the national level, these four traits appear to display statistical independence (Norris 1987). Although we do not claim that this list exhausts the inventory of potential contextual influences, these four dimensions do tap into the major influences suggested to date and will, therefore, constitute our empirical focus.

DATA AND METHODS

In order to assess these rather different predictions we have constructed a data set combining survey data with social indicators collected at the subnational level (usually provinces or an equivalent administrative unit). In this way, we have a measure of the local social structure hypothesized to affect each person surveyed.

We start with data from the 1983 Euro-barometer that we have examined (Banaszak and Plutzer n.d.), as have others (e.g., Klein 1987; Norris 1987; Wilcox 1991). This permits us to begin with baseline multivariate models of individual-level effects comparable to much of the literature. The Euro-barometer 19 survey, done in 1983, focuses on the role of women in society and is part of an ongoing survey series conducted by the Commission of the European Communities within member nations.

Since the national sample sizes are not proportional to the actual relative sizes of the national populations, we weight the data as recommended by the principal investigators (Rabier, Riffault, and Inglehart 1984). This weight adjusts the national samples to produce pooled male and female subsamples that are representative of the European Community. Pooling national samples makes sense in the present analysis, for the strong contextual hypothesis is that the nations differ only in their social structure. Pooling samples permits us to see whether any residual national differences remain after controlling for the effects of social structure.

The European Community nations included in the present analysis are Belgium, France, Germany, Ireland, Italy, Luxembourg, the Netherlands, and the United Kingdom. For a variety of reasons we reluctantly exclude respondents from Greece in the present study (a decision also reached by Wilcox 1991). In personal communications with us, several researchers expressed concern about the validity of the Greek data, since Greek men and women rate as most feminist on our measure but near the bottom on several variables concerning women's occupational roles. More importantly, we were unable to construct regional indicators for several of our key contextual variables because of missing data on some measures and an incompatibility of regional definitions between the Euro-barometer and Eurostat's (1984) *Yearbook of Regional Statistics.* This made it impossible to match regional data with most Greek respondents precisely.

Dependent Variable

Attitudes toward feminism are measured by a scale created by summing the answers to six questions concerning the women's movement and its goals. Respondents were asked to locate themselves on a four-point scale (scored here from 0 to 3), ranging from *completely disagree* to *completely agree* with the goals of (1) fighting against prejudiced people who would like to keep women in a subordinate role, (2) obtaining "true equality between women and men in their work in careers," (3) persuading political parties give women equal opportunities to be party leaders or candidates, (4) arranging gender equality in responsibilities for child care, and (5) organizing "women into an independent movement to achieve a radical transformation of society." In addition, answers to a question asking if respondents had a unfavorable or favorable opinion of the women's movement (also scored from 0 to 3) are included, as well. Appendix A contains the exact wording of these questions. Possible scores range from 0 for those taking the antifeminist position on all six questions to 18 for those consistently supporting the women's movement. For the 3,196 men and 3,230 women aged 18 or older who had valid scores on the profeminism scale, the means were 10.9 (s.d. = 3.65) and 11.8 (s.d. = 3.62) respectively.

The scale has high internal consistency, as demonstrated by Cronbach's alpha equal to .72, and has high predictive validity, as indicated by a strong positive association with women's reported membership in feminist organizations. (Over 87% of members, and 84% of those who would consider membership, score above the mean on the feminist goals scale). Exploratory factor analyses also suggest that these six items represent a single dimension (for details, see Appendix B). In light of the fact that studies have produced similar findings even when the content of scale questions only partially overlapped (compare, e.g., Banaszak and Leighley 1991; Davis and Robinson 1991; and Plutzer 1988), we feel that the present measure is an adequate measure of feminist support.[2]

Individual-level Independent Variables

Our set of individual-level variables is culled from the extant literature. In order to undertake a stringent test of the contextual and status discontent hypotheses, we want to be certain to include all known individual-level predictors of feminist support. We therefore employ measures of age, education, employment, income, marital status, number of children, attitudes toward women working, partisan preference, and urbanization in our analysis.

Religiosity, party affiliation, urbanization, marital status, and women's employment status are all measured by a series of dummy variables. Religiosity is coded as two dummy variables representing answers to the question, "Independently of whether you go to church or not, would you say you are a religious person, not a religious person, or a convinced atheist?"—the omitted category being atheists.[3] Political party affiliation is coded 1 if respondents indicated their intent to cast a vote for a leftist party in the next election.[4] Urbanization is measured by two dummy variables indicating whether the respondent lives in a rural area or a medium-size town—the omitted category being citydwellers.

For women, marital status is measured by a series of dummy variables representing respondents who are married, divorced or separated, widowed, and never married (with the latter category the omitted group in regression models). Women's employment statuses are coded as dummy variables representing whether the respondent works full-time, part-time, is unemployed and looking for work, or is not in the labor force (the omitted category).

As in our other study (Banaszak and Plutzer n.d.), we use a somewhat different classification scheme for men. It combines marital status and spouse's employment status. Men can be (1) single, (2) divorced, (3) widowed, (4) cohabitating, (5) married with wife working full-time, (6) married with wife working part-time, (7) married with wife looking for paid employment, or (8) married with wife not in the labor force. In combining marital status and spouse's employment, we essentially classify men by the traditionalism of their family life-style.

Education is measured by a nine-category variable denoting the age that the respondent finished full-time schooling. It is coded from 1, for leaving school at 14 and under, 9, for those studying until 22 years old or beyond. In addition, the number of children under 16 who are living at home is included as an indicator of traditional family life-style. We also have a measure of family income that groups respondents into national income quartiles. Finally, we incorporate the respondent's age and a series of dummy variables representing each of the nations in the study and Northern Ireland (France omitted). Northern Ireland is separated from Great Britain because its culture and context differs considerably from that of Britain.

Because commitment toward women working outside the home influences feminist support in both women and men (Banaszak and Plutzer n.d.; Gerson 1987) we also include a measure of the respondent's preferences toward women's employment. This attitudinal variable asks men, "If you had the choice, would you prefer your wife be in paid employment?" and asks women whether they would prefer to work if they had the choice. In a sense, the variable measures both men's and women's ideal role of the woman of the house.

Measures of Social Context

We utilize four measures of local culture and social structure. Two are derived from statistics reported in

Eurostat's (1984) *Yearbook of Regional Statistics*. Eurostat is the statistical office of the European Community and reports social and economic indicators at the level of 116 "administrative units." These usually correspond to official civil subdivisions, such as *Länder*, provinces, and the like. These regions vary substantially in size and population, averaging about three million people each. Although more precise estimates of the effect of social structure might be derived from data based on towns or neighborhoods, these represent the finest level of analysis possible at present. Compared to an analysis of nations, these provide more homogeneous social settings and sufficient cross-region variation (because of a large enough number of units) to undertake multivariate analyses.

The first social context measure is the ratio of the female economic activity rate to the corresponding rate for men. The sex-specific activity rates are computed as the number of women and men employed full-time divided by the total adult female and male populations, respectively. The logic of using a female-to-male ratio of rates is that female activity rate may reflect national unemployment trends, as well as women's status. The rate ratio provides an indicator of women's economic participation relative to men's.

In a similar fashion, we calculate a female-to-male rate ratio of third-level students. This second context measure indicates the relative status and opportunities for women to pursue college-level education and training.

The last structural measures are the regional divorce rates and religiosity levels. Because these could not be derived from archival sources at the regional level, they are calculated from Euro-barometers 9–18 (1978–82). We pooled these 10 surveys and aggregated individuals according to region of residence. This allowed us to calculate regional rates based on answers from a total of over 125,000 individuals. Therefore even in very small regions, we were able to compute divorce rates and religiosity measures from a large number of respondents. The average regional sample size from which the divorce rate is calculated was 1,480, with the contextual variable for 95% of the respondents based on regional samples of 242 or more. Because the religiosity item was not asked on a regular basis, the regional samples for this measure were somewhat smaller, but still large enough for fairly reliable measurement. The contextual measures of religiosity were based on an average of 336 individual respondents with 90% based on 54 or more individuals.

The divorce rate is simply the percentage of those from a given region who identify themselves as divorced. The religiosity context measure is coded as the mean within an area based on answers to a question asking respondents, "Do you personally feel, irrespective of how often you go to church, that your religion is of great importance, some importance, or only of little importance in your life?"—coded 1, 2, and 3, respectively.

In addition, we computed several cross-product interaction terms as suggested by the status discontent theory outlined above.

Statistical Methods

We estimated a series of ordinary least squares regression models in which the profeminism scale is the dependent variable. Each model is estimated separately for men and women on theoretical grounds (since Klein [1984] argues that feminist consciousness develops differently in men and women) and in order properly to specify the regression models for men that require a measure of *spouse's* employment status. Our first model includes only individual-level variables plus the dummy variables denoting nation. The second model adds the four measures of social context. Succeeding models add one interaction term each. Although only a model with all hypothesized interaction terms is correctly specified, we report these intermediate models because multicollinearity is often a problem with interaction terms. In such cases, inspection of intermediate models is often helpful. In the present case, however, the fully specified model produces interaction estimates virtually identical to those in the more comprehensive models. Finally, all significant interactions are included in a final model.

RESULTS

The results from the statistical analysis for women are reported in Table 1. The first model simply includes individual-level main effects. The findings are nearly identical to results reported by Wilcox (1991), and we report them without further comment except to note that the national differences reflected in model 1 are not substantially changed from a model without any individual-level regressors.

We turn first, then, to an exploration of the influence of structural variables in the absence of interaction effects. These can be found in Table 1 column 2. The first important finding is that the addition of the context measures does not alter the significance or sign of any individual level effects in model 1; that is, the individual-level effects identified by previous researchers are not spurious due to the effects of individual's social environments.

We find that three of the four contextual measures are significant predictors of scores on the profeminism scale. The nonsignificant variable is the mean religiosity of the region. We note that we also found no effect of a similarly constructed measure of average frequency of church attendance.

Women's educational achievement in a respondent's region has a positive effect on profeminist beliefs. Living in areas where women's college enrollment approaches that of men increases a woman's feminist beliefs after taking into account the positive effect of her own educational attainment.

We were somewhat surprised, however, to find that the extent of women's employment and the

TABLE 1

Regression of Profeminism Scale, Female Subsample

INDEPENDENT VARIABLE	MODELS 1	2	3	4	5	6	7	8
Intercept	12.028*	11.363*	11.381*	9.082*	11.883*	10.413*	9.492*	8.872*
Country								
Belgium	−.225	−.403	−.410	−.460	−.395	−.395	−.464	−.458
Netherlands	−1.367*	−1.520*	−1.534*	−1.670*	−1.505*	−1.506*	−1.683*	−1.673*
West Germany	−.061	.527	.511	.487	.524	.536	.463	.469
Italy	−.188	−.424	−.446	−.456	−.427	−.439	−.484	−.493
Luxembourg	.289	.177	.170	.141	.173	.185	.124	.129
Denmark	.116	−.208	−.195	−.136	−.243	−.216	−.162	−.168
Ireland	−.067	−.586	−.596	−.711	−.574	−.592	−.720	−.723
Great Britain	−1.430*	−1.175*	−1.159*	−1.208*	−1.159*	−1.156*	−1.178*	−1.166*
Northern Ireland	−1.617	−1.680	−1.663	−1.775	−1.666	−1.683	−1.752	−1.754
Education Level	.162*	.174*	.174*	.891*	.175*	.173*	.990*	.987*
R is religious	−.812*	−.868*	−.874*	−.887*	−.870*	.366	−.899*	−.089
R is not religious	−.289	−.349	−.355	−.360	−.342	−.327	−.358	−.344
Currently married	−.061	−.037	−.025	−.011	−.062	−.040	−.005	−.007
Cohabitating	.581	.673	.675	.746	.609	.667	.671	.668
Divorced or seperated	.357	.486	−.067	.577	.438	.479	.037	.041
Widowed	−.285	−.203	−.183	−.133	−.213	−.198	−.119	−.117
Number of children under 16	.006	−.005	−.009	−.005	−.006	−.005	−.011	−.011
Woman works full-time	.459*	.492*	.498*	.479*	−.917	.495*	−1.425*	−1.413*
Woman works part-time	.295	.314	.303	.317	−1.041	.314	−1.528*	−1.518*
Woman looking for work	.523*	.547*	.577*	.518*	−.806	.549*	−1.295*	−1.284*
Should woman of house work?	.677*	.646*	.633*	.636*	.672*	.648*	.657*	.658*
Exact age	−.031*	−.030*	−.030*	−.030*	−.029*	−.030*	−.029*	−.029*
Intends to vote for Left party	.516*	.518*	.520*	.499*	.519*	.516*	.499*	.498*
Family income quartiles	.085	.107	.107	.108	.113	.107	.117	.117
Rural place of residence	.145	.020	.007	.021	.031	.025	.026	.029
Medium-size town	.473*	.390*	.388*	.392*	.390*	.394*	.390*	.393*
Female-to-male activity rate ratio 1982	—	−2.464*	−2.520*	−2.444*	−3.958*	−2.479*	−4.517*	−4.515*
Female-to-male student rate ratio 1979	—	4.630*	4.604*	10.383*	4.689*	4.791*	11.222*	11.307*
Divorce rate 1979–82	—	−19.706*	−21.477*	−20.502*	−19.949*	−20.024*	−22.502*	−22.675*
Mean religiosity (hi = not religious)	—	.005	.040	−.077	.020	.367	−.038	.199
Divorced × divorce rate	—	—	22.491†	—	—	—	19.848	19.459
Education × student rate ratio	—	—	—	−1.637*	—	—	−1.860*	−1.854*
Active × activity rate	—	—	—	—	3.284*	—	4.450*	4.426*
Religious × Religiosity mean	—	—	—	—	—	−.507	—	−.333
Adjusted R-squared	.159	.164	.164	.168	.165	.164	.171	.171

Note: Omitted categories for dummy variable sets are *France, not in labor force, single, atheists,* and *citydwellers.*

Sources: For individual level variables, Euro-barometer 19 (Rabier, Riffault, and Inglehart 1984); for contextual variables, Euro-barometer cumulative file and *Yearbook of Regional Statistics* (Eurostat 1984).

*p < .05, two-tailed test.

†p < .05, one-tailed test.

regional divorce rate are negatively associated with the dependent variable. According to this analysis, individuals less frequently support feminist goals when they live in an area where a large percentage of women work or where there is a high divorce rate. Diagnostic calculations (not reported here) show that these negative findings are neither a result of multicollinearity among context measures nor created by inclusion of the various individual-level variables. The same results are obtained when employment rate and divorce rate are entered singly into the initial model. Nor, as we shall see, are the signs of these coefficients altered when the interaction terms are entered into the model. In short, these findings are robust and resisted our several attempts to explain them away statistically. We also note that while three contextual measures explain some of the variance in profeminism scores, the explanatory power of nation is undiminished.

Models 3–6 each adds a single interaction term to

FIGURE 1

Joint Effects of Women's Work Status and Regional Female Labor Force Participation Ratio on Feminist Attitudes

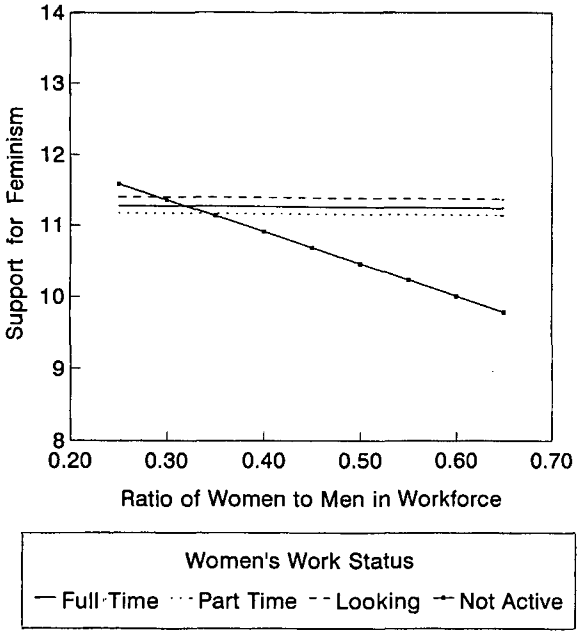
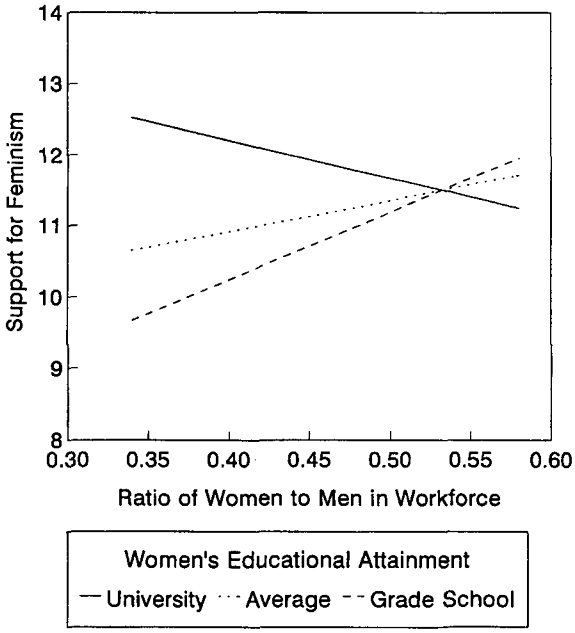


FIGURE 2

Joint Effects of Women's Education and Regional Female Education Ratio on Feminist Attitudes



model 2. The estimates for model 3 show that religiosity is not a significant predictor either as a main effect or in interaction with individual religiosity. Model 6 shows that the negative effect of regional divorce rate does not vary between the divorced and the not divorced. On the other hand, models 4 and 5 show that the effect of women's employment and women's educational attainment depend on the corresponding individual traits. Model 7 includes both the interaction terms that were significant individually; and we shall elaborate on the signs and interpretations of the interactions based on the estimates from this final model. We note, however, that contextual effects and interactions do not explain away national differences in any of the models.

The only way to understand the signs and magnitudes of the interaction and individual-level effects (which change substantially with the inclusion of interactions) is to examine the cumulative effect of individual, context, and interaction. To facilitate this, we have calculated these cumulative effects for each interaction set while holding the values of all other variables constant at their mean values. These calculations are represented graphically in Figures 1 and 2.

Figure 1 shows how the effect of regional rates of female labor force participation differ among women depending on their own employment status. In particular, the overall regional levels of female employment make no difference for women in the labor force but have a marked effect on women completely outside the labor market. The pattern is consistent with the status discontent hypothesis: domestic women become increasingly conservative as more of their female neighbors become economically active.

The results from this model also help us make sense of the overall negative effect of women's activity rate seen in model 2. Since the majority of women in Western Europe are not economically active, their negative reaction to women's entry into the labor force drives the main effect of this variable when we do not include an interaction term.

The effect of education is very different from the effect of female participation in the paid labor force. Women with little education are likely to exhibit high levels of feminist support when living in an area where relatively many women enter universities or advanced technical schools. However, the interaction term is negative, indicating that the effect of context decreases as women's own level of education increases. Figure 2 shows how the predicted level of profeminism varies according to regional rates of female college enrollment for three groups of women: those with only a grade school education (leaving school before age 14), those with university education (leaving at 22 or older) and those at the mean value (leaving school at about the age of 17). When we look at women who have completed many years of schooling, we find that living in an area with many well-educated women actually reduces their support for feminism slightly. However, the great majority of women never achieve that level of education, so the general effect of the educational opportunity for women is to raise women's support for feminism.

TABLE 2

Regression of Profeminism Scale, Male Subsample (2,483)

INDEPENDENT VARIABLE	MODELS 1	2	3	4	5	6	7	8
Intercept	11.931	8.554	8.556	7.665	6.026	8.894	6.332*	5.239
Country								
Belgium	−.339	−.123	−.123	−.112	−.159	−.101	−.137	−.124
Netherlands	−.701*.	−.294	−.293	−.279	−.468	−.327	−.510	−.494
West Germany	−1.273*	−.728*	−.728*	−.714	−.814	−.705	−.791*	−.774*
Italy	−.812*	−.405	−.405	−.411	−.422	−.397	−.414	−.423
Luxembourg	−.050	.156	.156	.166	.164	.162	.171	.182
Denmark	.226	−.212	−.212	−.204	−.169	−.236	−.195	−.184
Ireland	−.881	−.325	−.326	−.331	−.268	−.332	−.275	−.283
Great Britain	−.764*	−.290	−.290	−.253	−.294	−.262	−.263	−.220
Northern Ireland	−1.604	−.919	−.920	−.917	−.887	−.992	−.890	−.889
Education level	.082*	.083*	.083*	.082*	.766*	.083*	.785*	.794*
R is religious	−.435	−.406	−.406	−.912	−.410	−.400	−.404	−1.173
R is not religious	−.509*	−.496*	−.496*	−.479*	−.505*	−.478*	−.486*	−.466
Cohabitating	.387	.388	.387	.361	.390	.427	.433	.401
Divorced or seperated	−.964*	−.939	−.920	−.932	−.866	−.928	−.853	−.778
Widowed	.099	.113	.113	.109	.127	.109	.123	.117
Number of children under 16	−.232*	−.228*	−.228*	−.227*	−.232*	−.233*	−.237*	−.237*
Wife emp full-time	.762*	.714*	.714*	.707*	.709*	−.731	−.885	.884
Wife emp part-time	.422	.389	.389	.386	.418	−.984	−1.095	−1.090
Wife looking for work	1.464*	1.450*	1.450*	1.443*	1.398*	.014	−.187	−.186
Wife not in labor force	.497	.497	.497	.491	.514	.490	.506	.499
Should woman of house work?	.786*	.792*	.792*	.792*	.815*	.806*	.831*	.831*
Exact age	−.037*	−.038*	−.038*	−.038*	−.038*	−.038	−.038*	−.038*
Intends to vote for Left party	1.114*	1.124*	1.124*	1.125*	1.126*	1.103*	1.104*	1.105*
Family income quartiles	−.042	−.036	−.036	−.038	−.029	−.033	−.025	−.027
Rural place of residence	−.176	−.197	−.198	−.189	−.189	−.188	−.179	−.169
Medium-size town	.132	.160	.160	.167	.144	.150	.132	.138
Female-to-male activity rate ratio 1982	—	−1.170	−1.168	−1.220	−1.124	−2.022	−2.063	−2.110
Female-to-male student rate ratio 1979	—	5.710*	5.707*	5.847*	10.995*	5.729*	11.162*	11.391*
Divorce rate	—	−3.777	−3.765	−4.312	−4.565	−3.927	−4.752	−5.356
Mean religiosity (hi = not religious)	—	.482	.481	.835	.586	.466	.571	.992
Divorced × divorce rate	—	—	−.902	—	—	—	—	−3.348
Religious × religiosity mean	—	—	—	−.551	—	—	—	−.659
Education × student rate ratio	—	—	—	—	−1.573*	—	−1.616*	−1.638*
Active × activity rate ratio	—	—	—	—	—	3.369†	3.715*	3.692*
Adjusted R-squared	.111	.112	.112	.112	.117	.113	.118	.118

Note: Omitted categories for dummy variable sets are *France, not in labor force, single, atheists,* and *citydwellers.*

Sources: for individual level variables, Eurobarometer 19 (Rabier, Riffault, and Inglehart 1984); for contextual variables, Eurobarometer cumulative file and *Yearbook of Regional Statistics* (Eurostat 1984).

$^{*}p < .05$, two-tailed test.

$^{\dagger}p < .05$, one-tailed test.

The men's results are presented in Table 2. As with the female subsample, the first equation is a baseline model of individual effects and yields results virtually identical to those reported by previous studies. Model 2 adds the main effect of the four measures of social structure. The only one of these variables to achieve statistical significance is the ratio of female to male college enrollment. Its effect is in the same direction as that for women: the more women enrolled in higher education in a man's region of residence, the more supportive he is likely to be of the feminist agenda. Again, this effect is above and beyond the effect a man's own educational attainment.

The most important finding here is perhaps that the cumulative effect of the nine national dummy variables, evaluated by a joint F-test, is now insignificant. (In model 1, F = 4.49 with 9 and 2,456 degrees of freedom; in contrast, the corresponding F for model 2 is an insignificant .79 with 9 and 2,452 degrees of freedom). Only the variable denoting West Germany achieves significance—and then not in every model tested.

When we add the interaction effects one at a time, we find that both the religion and divorce interaction terms are insignificant in all models. The activity rate interaction is significant in a one-tailed test in model 6. This should be interpreted with caution, since we did not have a strong hypothesis predicting the sign of this variable. However, in the models that include other interactions (i.e., models 7 and 8), this variable becomes consistently significant even though the joint F-test of it and its contituent main effects remains insignificant. Thus, we report these estimates for the record only and note that the pattern is virtually identical to that reported for women: men married to full-time homemakers seem to react against the increased female labor force participation by expressing antifeminist attitudes; others seem to be unaffected.

Only the interaction between men's personal educational attainment and the regional ratio of female to male college enrollment achieves statistical significance. As in the case of women, the interaction is negative, indicating that the effect of female college enrollment is greatest on the men with the lowest levels of education. The combined pattern of effects approximates that illustrated in Figure 2.

DISCUSSION

Previous studies of feminism identified several aspects of social structure as being important in the development of feminist consciousness or profeminist sentiment among men and women. In its strongest versions, this hypothesis argues that observed national differences are caused entirely by corresponding differences in women's status and opportunity structure. Yet these same studies focused entirely on either individuals or on nations and were, therefore, unable to test these contextual theories rigorously.

The absence of longitudinal attitudinal data prevents the type of genuinely causal analysis these theories ultimately require, but our cross-sectional data permit a much better assessment of these approaches than has been attempted before. We find that the evidence yields a somewhat more complex picture than we anticipated. We shall discuss our results, emphasizing their implications for the refinement of contextual theories of gender politics.

The results of our analysis of men's attitudes are easiest to interpret. Regional levels of women's enrollment in higher education are associated with profeminist opinions, and the effect is strongest among men with low and modest levels of education themselves. Moreover, these differences in women's education rates render the combined effects of the *nation* variables statistically insignificant, explaining away virtually all observed cross-national variation in men's support of feminism. Women's labor force participation, on the other hand, produces neither a progressive or reactive response from men.

In contrast to the findings for men, neither contextual variables nor interaction effects substantially reduced the effects of the nation on women's attitudes. This is a contrast we cannot explain and certainly merits attention in future work. Among the most surprising findings for women is that in areas where women's participation in the paid labor force was highest, individuals tended to adopt the most conservative values. This pattern, it turns out, is due to a conservative reaction on the part of those women who are not in the paid labor force. This appears to be a classic example of status politics, with those being left behind by rapid social change adopting an especially conservative posture.

We should point out that our estimates *do* confirm that individual participation in the labor force is associated with profeminist attitudes, so the effect of these structural changes is to provide "consciousness-raising" experiences for women who enter the labor force. However, the effect of an individual's paid employment is the same, regardless of the prevailing extent of female employment. Regional variation in women's employment is relevant only for nonactive women. These individual and contextual processes tend to cancel each other out, for simple zero-order correlation between regional female employment rate and profeminism is very small ($r = .10$).[5]

The total effect of women's educational achievement, on the other hand, has a strong and positive effect on individual's attitudes toward feminism. As in the case of labor force participation, women at the highest levels of achievement do not become even more feminist as local female enrollments increases. Yet the effect of local educational context is pronounced for women who lack university or professional training. Our findings show that education's main positive impact is not, as some have suspected, as a catalyst for women's entry in the work force. Rather, it appears to have a *direct* effect of its own. The literature offers only a few hints as to the precise social processes that might account for this finding.

One possibility is that women of high educational achievement represent more successful role models than women who enter the labor force. Given the barriers to women's advancement in the corporate world, this would not be terribly surprising. Comparative case studies using qualitative or social–psychological methods might shed some light on this question. Yet the role model perspective cannot easily explain why women's education's has a nearly identical influence on men.

Alternatively, Guttentag and Secord (1983) argue that educational opportunities for women create a cadre of women with the resources to counter those who advance and reenforce traditional gender roles or otherwise block women's opportunities. Similarly, a large pool of educated women can use their resources to create public support for the women's movement—its organizations and goals. Whatever the mechanism, it is clear at both the individual and aggregate level that women's access to higher education is the most important structural variable related to profeminist support by men and women.

The somewhat different effects of education and labor force participation also have some interesting consequences for political conflict. Education's tendency to move both the educated (through individual effects) and the less educated (through contextual effects) in the same direction promotes feminist goals with a minimum of conflict. Women's entry into the labor force, on the other hand, amplifies differences in feminist attitudes. In regions where large numbers of women are in the labor force, individual effects will increase feminist support among employed women but decrease it among those who are committed to domestic life-styles. Thus, areas where women have made the greatest gains in employment should be those where gender politics is most divisive and competitive.

This leads to a final question: Why has women's entry into paid labor force not led to uniformly profeminist attitudes as we find for women's entry into higher education? Part of the answer may be economic competition. Equal access to higher education poses no immediate economic threat to men and wives of male breadwinners. Yet as women press for equal pay, businesses may be unwilling or unable to pay men a "family wage" capable of supporting a family. Thus, the life-style and economic security of full-time homemakers may be materially threatened by other women's economic gains.

On the other hand, part of the answer may lie in the fact that the quality of women's employment remains well below that of equally qualified men. Women tend to have limited upward mobility, limited access to mentors or inside information, and lower pay. Moreover, feminism has had its most visible impact in the professions, rather than in industrial and clerical labor, where the great majority of women work. Most employed women occupy low-paying jobs with limited opportunities for upward mobility. These "gains" by women may provide neither positive role models nor a cadre of women with resources that can be used for political ends. The current character of "women's work" may therefore blunt its potential positive impact on other women.

Our findings suggest that the context of social structure is important in shaping the conflict surrounding the contemporary women's movement and the advancement of feminist goals. Moreover, individuals' reactions to their social environment depends on their own status. Further pursuit of contextual theories of political conflict is clearly warranted; and the results suggest a need for reassessing the political implications of women's increasingly strong presence in the paid labor forces of the Western democracies.

APPENDIX A

We present the original variable numbers, mnemonics, and exact wording of items comprising the Profeminism Scale.

V111 "What is your opinion of movements which have come about recently whose aim is the liberation of women?" (OPINMVT) Responses were recoded to 3 = *very high opinion*, 0 = *very bad opinion*.

Responses to the following statements were recoded to 3 = *completely agree*, 0 = *disagree completely*: "Do you agree or disagree with the aim to

V113 fight against prejudiced people who would like to keep women in a subordinate role both in the family and in society?" (AIMFIGHT)

V114 obtain true equality between women and men in their work in careers?" (AIMJOBEQ)

V115 persuade the political parties to give women the same chances as men of reaching responsible positions in the parties and of becoming candidates in elections?" (AIMPOLEQ)

V116 arrange things so that when a child is unwell it could be either the father or the mother who stays home to care for it?" (AIMCARE)

V118 organize women into an independent movement to achieve a radical transformation of society?" (AIMCHANG)

APPENDIX B

Table A-1 summarizes exploratory factor analysis and item analysis for Profeminism Scale items.

TABLE A-1

EXPLORATORY FACTOR ANALYSIS		ITEM ANALYSIS	
PRINCIPAL COMPONENT	EIGEN-VALUE	VARIABLE	FACTOR LOADING
1	2.594	V111 OPINMVT	.652
2	.967	V113 AIMFIGHT	.647
3	.741	V114 AIMJOBEQ	.752
4	.635	V115 AIMPOLEQ	.714
5	.594	V116 AIMCARE	.638
6	.470	V118 AIMCHANG	.520

Note: Cronbach's alpha = .721; standardized alpha = .728; alpha if V118 is removed = .716. V118 is the logical candidate to remove from the scale because it has the lowest factor loading and item-to-total correlation. We retain the item because its presence tends to increase the reliability.

Notes

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1. Similar arguments are also found in studies of women's movements and women's organizations. See, e.g., Chafetz and Dworkin 1986; Dahlerup 1986; Katzenstein and Mueller 1987.

2. A more detailed discussion of the properties of this scale can be found in Banaszak and Plutzer n.d.

3. Unfortunately, Euro-barometer 19 did not ask respondents to identify their religious affiliation. We were, however, able to explore the differences between Catholics and Protestants using data from Euro-barometer 8. Using a gender-roles scale as the dependent variable (the closest proxy for our profeminism scale), we found that religious affiliation made no difference in a sample of all European Community respondents. In light of this finding and of the fact that several studies show religiosity to be far more important than affiliation (the review essay Himmelstein 1986), we feel confident that the omission of religious affiliation from the present analysis is unlikely to bias our findings.

4. A detailed description of the codes in each country is available on request. They are based on Rabier, Riffault, and Inglehart's (1984) party codes. We choose not to include a general measure of left–right ideology, since it may be interpretable as a summary measure of political orientations and therefore determined in part by feminist orientation.

5. This finding does not rule out the possibility that increases in women's labor force participation *over time* promote profeminist sentiment. Yet it is impossible to examine this theory with the available data.

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Lee Ann Banaszak and Eric Plutzer are Assistant Professors of Political Science, Iowa State University, Ames, IA 50011.

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WHAT MOVES POLICY SENTIMENT?

ROBERT H. DURR *Washington University, St. Louis*

In spite of the fact that political eras in the United States are widely (and often ambiguously) defined in terms of a general policy sentiment or mood, political scientists have done little in the way of rigorous analysis regarding this subject. I argue that shifts in domestic policy sentiment along a liberal–conservative continuum may be understood in part as responses to changing economic expectations. Specifically, expectations of a strong economy result in greater support for liberal domestic policies, whereas anticipation of declining economic conditions pushes the national policy mood to the right. Using quarterly data for the period 1968–88, I present a multiple-time-series error correction model that lends considerable support to the hypothesis.

For years, analysts of American politics have speculated about the national political mood. Conceived of on a liberal–conservative continuum, the mood of the United States has served as a convenient method for classifying eras, distinguishing, for example, between the "liberal" 1960s and the "conservative" 1980s. Such discourse, grounded most often in nothing more than conventional wisdom or folklore, has been virtually unable to evolve beyond the level of mere speculation, due in no small part to the apparent inability of analysts to capture their elusive subject empirically. Nonetheless, theorists have wondered aloud about both the causes and the consequences of a shifting national mood (see, e.g., Kingdon 1984). If the American public of the 1960s was indeed more liberal, how did such a phenomenon come about? What caused citizens to adopt liberal domestic policies? Conversely, what pushed the United States back to the right in the Reagan era? Are such swings in national mood random? Or might they be explained as systematic responses to shifting political and economic landscapes? In short, what moves public opinion?

Economic factors, in general, have long been primary suspects (e.g., Dolbeare 1986; Edsall 1984; Schlesinger 1986) and are fingered once again in this analysis. Specifically, I argue that shifts in U.S. domestic policy sentiment may be understood in part as responses to changing economic expectations embraced by the public. Buoyed by perceptions of high (or increasing) economic security down the road, Americans are more likely to underwrite a liberal policy agenda. Anticipation of doom and gloom, on the other hand, stifles such a willingness, pushing the national policy mood to the right. In order to demonstrate the validity of this hypothesis, I employ a measure of policy sentiment developed by Stimson (1991a) and one of economic expectations in a multiple-time-series error correction model.

THE AMERICAN POLITY & ECONOMY

Particularly regarding the United States, it is difficult to consider political phenomena divorced from economic matters. The connection is clearly noted in the research of Campbell and his colleagues, who observe (and proceed to demonstrate) that "economic interest has long been seen as a primary motive impelling political action" (1960, 381). Subsequent empirical analyses concerned with the relationship between economic and political factors in the United States constitute one of the discipline's greatest growth industries in recent years. Demand-side perspectives, concerned with the degrees to which economic matters enter into the political-behavioral decision calculus of the American citizen, have focused attention upon election outcomes (Chappell and Keech 1985; Fiorina 1978; Kinder and Kiewiet 1981; Kuklinski and West 1981; Lewis-Beck 1988a), presidential approval ratings (Kernell 1978; MacKuen 1983; MacKuen, Erikson, and Stimson 1989; Mueller 1970, 1973; Norpoth and Yantek 1983; Shapiro and Conforto 1980), and macropartisanship (Lockerbie 1989; MacKuen, Erikson, and Stimson 1989). Theorists adopting a supply-side perspective have attempted to determine whether political parties and policymakers have sought to maximize political gains by meeting the voters' demands. Hibbs (1977) demonstrates that in both the United States and Great Britain, unemployment and inflation rates correspond to the particular political party (liberal or conservative) currently in power. In addition, a significant number of analysts have offered empirical evidence on the question of political business cycles—some finding evidence (Haynes and Stone 1988; Nordhaus 1975; Tufte 1978), some negative (Alt and Chrystal 1983; Beck 1982; Thompson and Zuk 1983), and at least one mixed (Williams 1990). If one is content to use as a crude measure of interest the sheer number of empirical analyses concerned with the role of economic phenomena in the American political arena, it is clear that this subject has received a significant amount of attention within the discipline of political science.

In spite of this relatively large amount of attention, few have specifically addressed the link between economic conditions (or expectations) and macro domestic policy sentiment. It is only after examining the empirical analyses just discussed that two likely explanations emerge. First, quantitative analyses of economic conditions and political phenomena are nearly universally *longitudinal* in nature. Analysts have reasoned that economic factors often exert delayed or lagged effects on political behavior, and the capacity to develop such a model rests on the availability of longitudinal data. Until now, no sufficient measure of domestic policy sentiment has been available for such analyses. Second, few analysts have been able to agree on an appropriate measure of economic expectations. While many political scientists have been content to employ objective measures of the state of the economy in their analyses, others have argued that subjective measures provide a means of circumventing the assumption that citizens are both willing and able to perceive economic conditions. Still others have debated the relative value of measures that presume either a backward- or forward-looking citizenry: Are voters retrospective or prospective in their evaluations of the economy? Thus, even if one were able to represent the national policy mood empirically, the task of operationalizing economic expectations is not a simple matter.

ECONOMIC EXPECTATIONS AND DOMESTIC POLICY SENTIMENT

As noted, I am hypothesizing that perceptions of an imminent economic downturn will cause an aggregate shift toward a more conservative policy agenda. Conversely, expectations of a healthy (or improving) economy contribute to the willingness within the American public to underwrite a liberal domestic policy agenda. Central to this hypothesis is the fact that such an agenda does not come cheap. Since the New Deal era (and perhaps, to a greater extent, since the Great Society initiatives of Lyndon Johnson), the liberal policy agenda has explicitly cited the need for an active federal government as provider and protector of jobs, health care, schools, housing, civil rights, and the like. While liberal elites may push such policies at all times, sugh an agenda can only be supported by a healthy economy or (to be more accurate) by a populace enjoying the fruits of a healthy economy. Thus, perception of economic security pave the way for the implementation of rather expensive (liberal) domestic policies by fostering a willingness among the public to pay for such policies.

Does it make sense to anticipate that given a certain set of circumstances, individuals will part willingly with a portion of their income in order to contribute to the construction of a "better" society? In spite of a surprisingly healthy body of evidence which challenges the role of self-interest in economic and political behavior (see Citrin and Green 1990), it is generally assumed rational for individuals to prefer a greater quantity of some good to a lesser quantity—regardless of circumstance. This assumption suggests that citizens will always prefer greater wealth for themselves; yet it stands to reason that the utility derived by an individual from a bundle of goods is lessened if many such bundles are already in that individual's possession. In other words, it is reasonable to anticipate that a threshold may be met where the relative weight of the simple desire for "more" is lessened with respect to an individual's decision-making processes. The same principle applies to the nation–state: As feelings of economic security increase, the citizenry will be more willing to part with the nation's wealth toward the pursuit of other goals. Should the economy of a nation–state become sufficiently *threatened* that the citizens lose their sense of security and, by extension, their willingness to underwrite the government's pursuit of liberal policy goals, on the other hand, the public will demand a temporary suspension of such goals in favor of greater attention to the economy itself. To state the case simply, a concern for the oppressed—whether minorities and women or lakes and streams—can be suspended rather quickly when a typically secure populace suddenly feels that *it* is in danger of oppression.

The expectation that a populace enjoying the fruits of economic security will be more willing to support a liberal policy agenda, then, need not rest on the assumption that individuals will occasionally behave in an altruistic manner. Instead, the achievement of a sufficient level of economic comfort permits citizens to establish new objects of their self-interest. Individuals who support tax-and-spend policies designed to fight poverty may do so not because they are saintly but, rather, because they find the visual reminders of poverty around them particularly unpleasant. And when economic conditions permit, the achievement of such "social"—albeit still, perhaps, fundamentally self-interested—goals becomes possible.

Those comfortable within an economic paradigm will point out that the anticipated relationship between domestic policy sentiment and economic expectations is nicely captured by the principle of diminishing marginal turns. A nation with great collective wealth will value money less and will thus be more willing to part with it toward the attainment of other goals. Should money become scarce, however, its value relative to other goods (e.g., the utility gained by contributing to the betterment of strangers) increases. Given the fact that most liberal policies require a redistribution of wealth, one would expect that in times of economic uncertainty or insecurity, citizens will be less supportive of the liberal domestic policy agenda. Thus, the same anticipated relationship between economic expectations and domestic policy sentiment emerges: as economic expectations contribute to a greater sense of security, domestic policy sentiment will reflect a greater willingness to adopt liberal policies.[1]

Before the hypothesis tying economic expectations to domestic policy sentiment can be tested, it is necessary to return to questions of operationalization. After examining Stimson's measure of policy preferences, I develop a means of representing the aggregate economic outlook embraced by citizens of the United States.

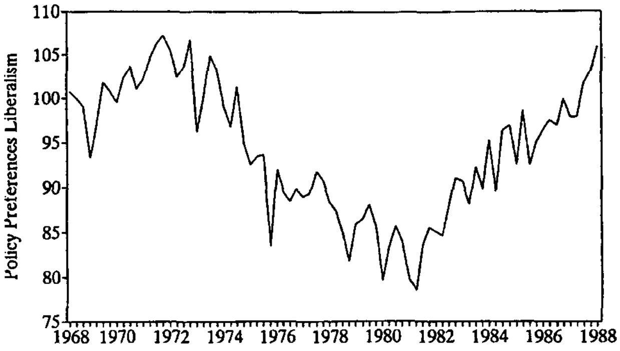
DEVELOPING A MEASURE OF DOMESTIC POLICY SENTIMENT

Toward an analysis of political eras and representation, Stimson (1991a) develops a "recursive dyadic dominance method" for the purpose of constructing a single time series of public opinion data based upon hundreds of distinct survey marginals. This modeling process, designed to circumvent the problems associated with the many missing values in single-issue time series of opinion poll data, permits the identifiction of the shared movements over time across all individual series. Consider, for example, two different time series, one measuring citizen's attitudes toward federal aid for education, and the other focusing on welfare programs. Although we expect each series to have a dynamic of its own, it is the shared movement over time along a liberal–conservative continuum that constitutes (a small part of) domestic policy sentiment. Given many more, distinct, issue-based time series, the identification of such shared variance might be as simple as computing the mean of all series at every time point. Unfortunately, large gaps in many of the issue time series prevent such an approach. Stimson argues that this problem may be eliminated by assuming that one needs only two administrations of a survey item to make an assessment about the relative "liberalness" of the one point in time to the other. For example, if we know that a higher percentage of individuals favored government aid to minorities in 1990 than in 1989, such information might contribute to the conclusion that domestic policy sentiment in 1990 was more liberal than it was the year before. Of course, such a shift in sentiment on only one issue does not constitute a whole-sale shift in domestic policy sentiment. But if a large number of similar movements in distinct policy arenas occur simultaneously, the argument for an aggregate (across issues) shift is strengthened considerably. In order to compare and calibrate issue marginals over time, Stimson (1991a) develops a dyadic recursion algorithm that successfully captures shared issue variance, or *domestic policy sentiment.*[2]

Composed of 60 different domestic policy issues with 786 measured values, the resulting domestic policy sentiment time series (aggregated to quarterly observations) is presented in Figure 1.[3] The time series is scaled so that greater values indicate liberal policy sentiment and lesser values suggest conservative sentiment (around a mean of 100). While the pattern exhibited is somewhat supportive of our conventional wisdom regarding a general pattern of the national mood, there are a few surprises. For example, Stimson's measure suggests that the conservatism we tend to associate with the decade of the 1980s reached its zenith not during Reagan's tenure but immediately prior. Indeed, the 1980s witnessed a steady shift in domestic policy preferences toward liberalism. As Stimson argues, the pattern exhibited by this time series is not merely the product of the method invoked in its construction but, rather, the data themselves. Throughout the years (spanning nearly two decades) that General Social Surveys have measured citizens' attitudes toward gun control, aid to the sick, the environment, education, and health care, 1989 saw the most liberal responses (i.e., the greatest percentages of respondents falling within categories representing liberal attitudes) for all these items. Conversely, 1980 produced the most conservative attitudes on the issues of gun control, the environment, and health care (Stimson 1991a).

FIGURE 1

U.S. Domestic Policy Sentiment, 1968:2–1988:1



The path that domestic policy sentiment follows through time appears, then, to be distinctly different from the courses taken by self-identified party identification and ideology.[4] Noting this distinction is important. It is not the purpose of this work to examine the phenomena that may cause party identification and ideology to change. Rather, my focus rests on the reasons that aggregate domestic policy sentiment (a largely latent phenomenon that, for obvious reasons, resists measurement via a single survey question) changes over time.

DEVELOPING A MEASURE OF ECONOMIC EXPECTATIONS

In order to operationalize economic expectations, a good starting point is the University of Michigan's Index of Consumer Sentiment (ICS). Composed of a battery of survey questions designed to tap public perceptions of the economy, the index contains one question, in particular, that appears to be well suited for the measurement of economic expectations. Focusing attention on long-term business conditions, the question reads, "Looking ahead, which would you say is more likely—that in the country as a whole we'll have continuous good times during the next five years or so, or that we'll have periods of widespread unemployment or depression, or what?" The result-

FIGURE 2

Domestic Policy Sentiment and 5-Year Business Expectations

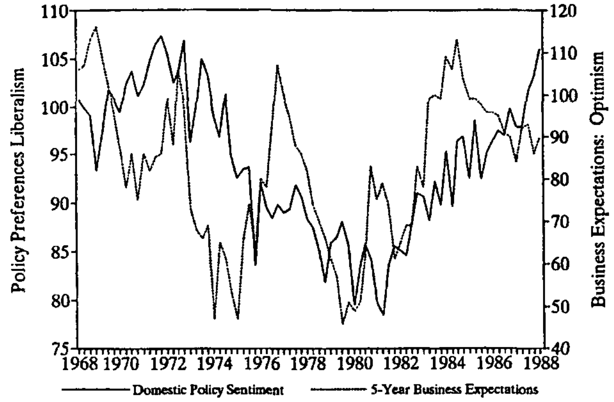
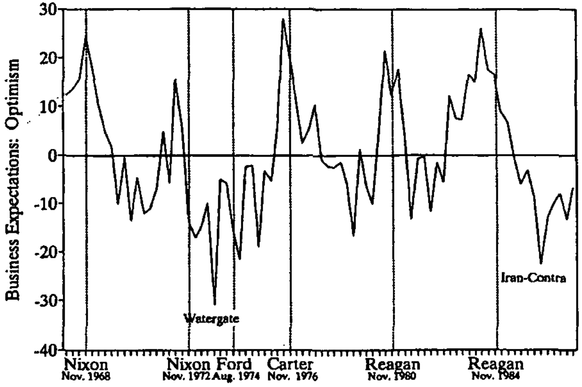


FIGURE 3

The "Political" Component of 5-Year Business Expectations

(Residuals from regression of business expectations on 4 standard measures of the economy)



ing time series of quarterly data, along with domestic policy sentiment, is presented in Figure 2.

Four virtues of the ICS long-term business expectations survey item for the present analysis merit attention. First, having available a measure of subjective evaluations of economic conditions renders unnecessary the development of a model both incorporating objective economic conditions and the assumption that such conditions are actually perceived. Second, the question focuses explicitly on prospective evaluations of the economy. A number of analysts have demonstrated that within the context of predicting voting behavior, survey instruments that tap forward-looking judgements tend to out-perform those that are retrospective in nature (Lewis-Beck 1988b; Kuklinski and West 1981). Third, the prospective orientation of the question is sufficiently long-term to warrant the presumption that it captures the somewhat more "ethereal" notion of economic security integral to the hypothesis under consideration. Asking individuals to assess the short-term economic future may miss the "bigger picture" of sentiment regarding the *overall* anticipated health of the economy.[5] Indeed, even well-trained economists are likely to hesitate before producing a prediction of economic conditions five years down the road. In lieu of more concrete indicators, I assume that they (as well as the average citizen) will rely on a somewhat more abstract assessment, grounded in feelings regarding the overall health of the economy. And finally, the related ICS has been shown to respond to the U.S. economy (Katona 1975). As preferable as subjective evaluations may be, the knowledge that such evaluations are a function of reality is comforting.

Economic reality, however, is not the only determinant of long-term business expectations. To demonstrate that this is the case, I have regressed the time series of survey data on four measures of the objective economy. The four independent variables are *the next quarter's* measures of unemployment, inflation, and coincidental indicators, as well as the current quarter's leading economic indicators.[6] If long-term business expectations are purely a function of economic considerations, the residuals from this model (reflecting, primarily, errors in citizen judgment) ought to be essentially random. If, on the other hand, noneconomic factors account for a meaningful portion of the variance in business expectations, the residuals are not likely to be distributed in a random fashion.

The residuals from the regression of long-term business expectations on the four measures of the economy are presented in Figure 3. To reiterate, this time series represents the portion of business expectations that is not accounted for by standard measures of the economy. Judging from the pattern exhibited, it is certain that something far more systematic than random errors of sampling or perception is at work. Indeed, the residuals suggest the existence of a *political* component of long-term business expectations. Peaks in the time series correspond well to presidential elections, suggesting that consumer sentiment, like presidential approval, is vulnerable to a "honeymoon" effect. And the two most prominent valleys correspond in time to the Watergate and Iran–Contra scandals. One would be hard-pressed to imagine a better-specified model (with additional or altogether different measures of the economy) capable of eliminating the pattern of residuals displayed in Figure 3.

Such results are not surprising. It is intuitively reasonable to expect that the public's assessment of future economic conditions is colored by the optimistic rhetoric of presidential campaigns. Just as approval ratings for presidents tend to be at their highest at the outset of a new term, so, too, is our confidence in the president's economic program. Viewed within this perspective, the low marks corresponding to Watergate and Iran–Contra are equally plausible. Ostrom and Smith (1990) demonstrate that the Iran–Contra affair cost Ronald Reagan nearly 20 points in presidential approval, and the effect of Watergate on Richard Nixon's approval ratings is legendary. Just as the electorate is prone to adopt an overly optimistic outlook regarding future conditions during the heady days of a new presidential administration, an overly pessimistic view may be embraced when the president's capacity to manage the economy is questioned.

Perhaps the strongest evidence in favor of the existence of a political component of long-term business expectations comes from a comparison of the residuals in Figure 3 with a time series measuring citizens' opinions of the government's economic policy. The latter measure may be found (along with the ICS) in Michigan's Survey of Consumer Attitudes. Asking respondents to assess the economic policies of the government, this item focuses quite literally on the "political" component of consumer expectations. If the residuals represent the same, the two time series ought to follow the same pattern over time, and they do. Peaks and valleys correspond nicely, producing a correlation of .50.

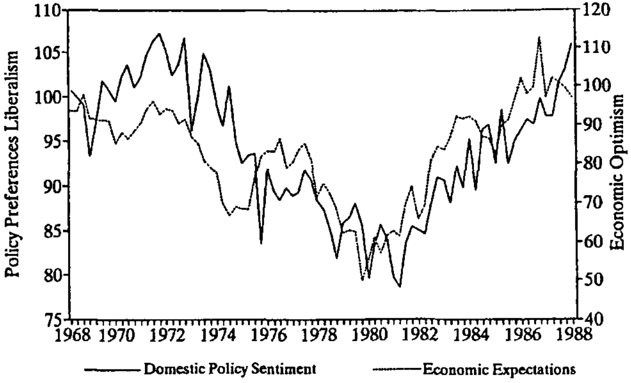
Isolating the *economic* portion of long-term business expectations (the ultimate goal of this exercise), then, is quite simple. One need only employ the predicted values generated by the regression model. The resulting time series represents business expectations purged of the distorting effects of political evaluations. To state it another way, subtracting the residuals generated by a model predicting business expectations on the basis of objective economic conditions from the original five year business expectations time series leaves us with that portion of business expectations attributable to the economy itself.[7] This "economic portion" of long-term busines expectations is well suited to serve as an operational representation of economic expectations. In addition to the four virtues of the original consumer sentiment series just discussed, this conception of economic expectations avoids the only drawback, namely, the distorting effect of political considerations. Figure 4 presents U.S. economic expectations along with domestic policy sentiment. Whereas policy sentiment and long-term business expectations covary to a degree (Figure 2), Figure 4 suggests the dramatic effect of purging political evaluations from the original measure of consumer sentiment. As hypothesized, liberal shifts in domestic policy sentiment are associated with increasingly optimistic economic expectations. The opposite also holds true: conservative domestic policy sentiment corresponds nicely with periods of relative pessimism regarding the future of the economy.[8] What remains, then, is the development of a model designed to identify the precise nature of the relationship between economic expectations and domestic policy sentiment.

CONTROLLING FOR OTHER EFFECTS

Before entertaining methodological concerns, it is necessary to examine additional factors that may account for shifts in aggregate domestic policy preferences. I have argued elsewhere (Durr 1992) that the effects of federal policy outcomes and a number of historical events on policy sentiment must be controlled prior to estimating the impact of economic expectations. I now present the rationale for such controls.

FIGURE 4

U.S. Domestic Policy Sentiment and Economic Expectations



Federal Policy Outcomes

In additions to economic expectations, policy preferences are also hypothesized to be a function of the public's assessment of federal policy outcomes. Policy outcomes viewed as overly "intrusive"into the daily lives of citizens will produce a call for a more limited government, whereas perceptions that the federal government is failing to do enough on behalf of its citizenry will lead to the expressed desire for a more active government. To the extent that calls for limitation or greater action are synonymous with shifts in policy sentiment in a conservative or liberal direction, then, the anticipated relationship between policy outcomes and policy preferences is negative. Once a threshold is met where citizens believe that the federal government has gone too far in one direction or another (i.e., policy outcomes are either too intrusive or not intrusive enough), public opinion will react by moving in an opposite direction in an effort to pull in the reins of policymakers.

Note that the hypothesized relationship between policy preferences and policy outcomes is nonconstant. Policy outcomes are only expected to produce negative reactions when they are perceived to be extreme. Otherwise, I anticipate no causal connection. It is also important to note the distinction between the behavior of policymakers and policy outcomes. The former, argued by Stimson (1991b) to be a function of policy preferences, ultimately results in actual policy outcomes experienced by citizens. Given the oft-cited incremental nature of the policy process, it is entirely conceivable that a family of policies enacted during a time characterized by a strongly supportive public mood might one day (perhaps many years down the road) be met with equally strong opposition.

In order to capture empirically the intrusive nature of general policy outcomes, I have focused on matters of federal spending. Budgets and taxes represent "authoritative allocations," the changes and ratios of which should capture the drift of public policy over time. To wit, four distinct categories of federal budgeting are examined: domestic spending (the percentage of total federal outlays dedicated to human resources), defense spending (100% minus the percentage of total federal outlays dedicated to defense), income tax rates for the wealthy (average income tax rate of all returns with adjusted gross income of one million or more 1986 dollars), and federal grants-in-aid to state and local governments (the percentage of total state and local revenues consisting of federal grants-in-aid). The defense measure is reflected so that increases and decreases are consistent with those of the other measures; that is, a higher percentage for any measure indicates (roughly) a more intrusive federal government. Should these elements of federal policymaking move together through time, one could argue that their shared pattern is indicative of the nature of the federal policy outcomes. Not surprisingly, they do move together. All four series increase steadily throughout the 1960s, reach plateaus in the mid-1970s, and reverse course at the outset of the 1980s.[9]

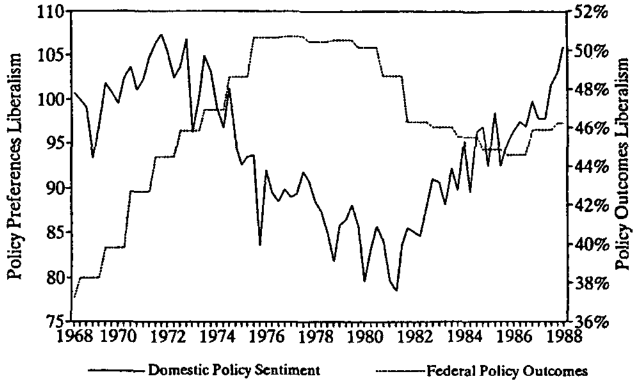
Figure 5 presents a measure of federal policy outcomes obtained by averaging the four time series just discussed.[10] Corresponding in time to the peak of policy intrusiveness is a dramatic conservative shift in policy preferences—precisely the type of negative reaction hypothesized. The visual evidence suggests that U.S. public opinion may have reacted negatively to policies perceived as overly intrusive. Such a reaction may have been due to the fact that at the same time many of the Great Society policies enacted in the 1960s were coming to full fruition, Americans experienced worldwide inflation. Although not unprecedented, Katona (1975) argues that this was the first instance that Americans began to doubt that resources could keep pace with worldwide demand. A natural reaction to this impression would be a desire for the U.S. government to roll back its reach and husband its resources. Such an event, then, may have served as a "trigger," touching off a negative reaction to the status quo of policy outcomes. Since policy outcomes are only presumed to affect policy sentiment in the wake of such a reaction, only that portion of the measure occurring after the outset of fiscal year 1975 (the first quarter of which is the front end of the plateau witnessed in Figure 5) is included in the analysis. Prior to fiscal year 1975:1, the measure takes the value of zero.[11]

Controlling for Other Effects

As noted, the measure of domestic policy sentiment under consideration consists of citizens' preferences across scores of distinct issue arenas. While preferences in any one issue arena are certain to be affected by many "garden-variety" political events, the list of such events capable of altering preferences across many such issues is likely to be small. Only four events during the two decades under analysis (1968–88) present themselves as viable candidates: the Vietnam War, Watergate, the Iran hostage crisis, and the Iran–Contra affair. Of these, only the first two seem to be of a sufficient magnitude to affect policy preferences; yet there is no reason not to include dummy variables to control for the possible effects of all four.[12]

FIGURE 5

Domestic Policy Sentiment and Federal Policy Outcomes



PURSUING AN APPROPRIATE MODELING STRATEGY

To recap, it is anicipated that domestic policy sentiment responds to changes in economic expectations. As the public becomes more (or less) secure regarding the future conditions of the economy, policy sentiment will shift in a liberal (or conservative) direction. In light of the operationalization of these phenomena, the statistical relationship is expected to be positive: increases in economic expectations and domestic policy sentiment indicate greater optimism vis-à-vis the economy and support for more liberal policies, respectively. Having seen (in Figure 4) that time series representing economic expectations and domestic policy sentiment do indeed track together through time, we may now turn to a more rigorous statistical analysis of the relationship between these two processes.

Both time-series analysts and casual observers will come away from Figures 4 and 5 with the impression that the realizations of domestic policy sentiment, economic expectations, and federal policy outcomes tend to drift in one direction or another for extended periods of time before reversing course. Statisticians refer to such time-serial patterns as "integrated" (or additive) processes. An integrated time series is one for which the value of any given point in time may be expressed as the sum of all past disturbances. Such a diagnosis is critical when choosing amont alternative modeling strategies. As Granger and Newbold (1974) demonstrate, the regression of one integrated time series on another is biased toward the false rejection of the null hypothesis. In other words, real-world relationships that may not exist will nonetheless be "found" in the data.

Fortunately, empirical tests are available for determining whether an individual time series is integrated. Referred to as the Dickey–Fuller (DF) and Augmented Dickey-Fuller (ADF) tests, these procedures go a long way toward eliminating the guesswork in a critical stage of time series analysis. (For a thorough and accessible discussion of these tests, see MacKinnon 1990.) In essence, the DF test for determining whether a time series is integrated involves the regression of first differences of the series on its lagged levels. If the series is stationary, the regression coefficient associated with the lagged levels will be negative and significant. The ADF test includes a number of lags of the first differences on the right-hand side of the regression equation and is appropriate if the resulting coefficients are significant (Engle and Granger 1987) or if the residuals from the DF regression are serially correlated (MacKinnon 1990). Critical values for these tests are greater than normal, due to nonstandard distributions, and are reported by MacKinnon (1990).

Based on the appearance of the time series presented in Figures 4 and 5, such tests are in order. But more importantly, are there a priori theoretical reasons to expect that domestic policy sentiment, economic expectations, and federal policy outcomes are integrated? Regarding domestic policy sentiment, it would be difficult to imagine the process as anything but integrated. As Stimson (1991a) notes, policy preferences reflect not just values but also strategic choices for the best way to achieve valued outcomes. The only way that citizens will know whether a given strategic policy preference is destined to succeed is via experimentation. In other words, the incremental trial-and-error process of policy implementation dictates that public opinion regarding the feasibility of some strategic choice will evolve slowly. Thus, it is unlikely that a general preference for liberal (or conservative) policies will reverse itself overnight. By the same token, economic expectations are not likely to fluctuate quickly around a fixed mean. In light of their relationship with economic conditions, the fact that most booms and busts come with sustaining heads of steam suggests that expectations will not shift from optimism to pessimism quickly. Finally, since federal policy outcomes (as operationalized above) are a function of the incremental policymaking process, such outcomes are integrated virtually by definition. It is well known, for example, that budgets are not cooked up from scratch. Instead, policymakers typically begin with last period's budget and make incremental changes as deemed necessary.

The results of Dickey–Fuller tests for the times series representing domestic policy sentiment, economic expectations, and federal policy outcomes are presented in Table 1. T-tests indicate that all three series have nonzero means in levels, suggesting the necessity of including constant terms in each DF regression. Note also that all three series have zero means after first-differencing, implying that all three exhibit zero mean drift. In each of the integration tests on levels, the constant term is nonsignificant, so that the inclusion of a linear time trend is unnecessary. For domestic policy sentiment, serial correlation of the error terms forced two lags of changes into the equation. In no case is it possible to reject the null of integration in levels; thus, all three series are integrated.

The order of integration is tested by examining the stationarity of each series after first-differencing. A time series that is integrated in levels but stationary in changes is first-order integrated (typically denoted I (1)). And as Table 1 demonstrates, domestic policy sentiment, economic expectations, and federal policy outcomes all exhibit stationary changes. Results of the Dickey–Fuller tests provide strong evidence, then, that all three time series are first-order integrated.[13]

A common solution to the "problem" of integration adopted by many analysts is to take first differences of a time series. Each time point is thus represented not as the *level* of the time series at time t but, rather, the *change* from $t - 1$ to t. This operation typically converts an integrated process into a stationary one, that is, a time series with finite variance around an identifiable mean. Although many analysts believe that relationships exist among different processes represented as levels ought to survive when expressed in terms of changes, King disagrees: "Differencing can cancel out the systematic component [relating one time series to another] . . . . Thus, as a nearly general rule, models based on differenced series tend to fit less and have higher standard errors and less stable coefficient values" (1989, 181). Indeed, a familiar cry among critics of "prewhitening" techniques is that we are "throwing out the baby with the bathwater."

In effect, modeling changes in the dependent measure as a function of (lagged) changes in the independent variables assumes that the effect of innovations in the explanatory series never last more than one time period. Consider, for example, a sharp one-quarter decline in economic expectations that is sustained indefinitely. If the hypothesized relationship between economic expectations and policy preferences is correct, such a shock should produce a conservative drift in policy sentiment. But if the movement to the right is not realized within a single three-month time period (i.e., if the resulting conservative shift takes place gradually over several quarters), a model based only on changes will miss the full response. Domestic policy sentiment may move in a conservative direction over a number of quarters consistent with the new pessimistic *level* of economic expectations, eventually achieving and maintaining an appropriate (conservative) level of its own. Viewing the hypothesized relationship between policy preferences and economic expectations in these terms (in which changes in policy sentiment are presumed

TABLE 1

Series Properties and Integration Tests

SERIES CHARACTERISTICS	DOMESTIC POLICY SENTIMENT	ECONOMIC EXPECTATIONS	FEDERAL POLICY OUTCOMES
Mean in levels	94.14	82.80	.30
T-test for mean = 0	113.03*	56.86*	11.75*
Mean in first differences	.07	.04	.01
T-test for mean = 0	.16	.07	.91
Integration test, levels[a]			
Constant	4.34	5.68	.02
Levels, lagged 1 period	−.05	−.07	−.07
T-statistic[b]	−.82	−1.61	−1.38
Changes, lagged 1 period	−.41*	—	—
Changes, lagged 2 periods	−.28*	—	—
Durbin–Watson	—	2.18	1.99
Breusch-Godfrey[c]	.66	—	—
Ljung–Box Q (24 d.f.)[d]	13.77	24.77	.98
Integration test, changes[e]			
Changes, lagged 1 period	−1.33	−1.13	−1.00
T-statistic[f]	−12.36**	−9.99**	−8.77**
Durbin–Watson	2.18	2.01	2.00
Ljung–Box Q (24 d.f.)	26.52	26.79	.10

Note: The Domestic Policy Sentiment index is composed of 60 different domestic policy issues. It is a quarterly series based on the index and method developed by Stimson (1991a) for annual data. See text and Note 3.

The Economic Expectations index is the predicted value from the regression of the 5-year business expectations measure (based on a question contained in the University of Michigan's Index of Consumer Sentiment) on 4 standard measures of the economy; leading economic indicators (current), and next quarter's unemployment, inflation, and coincidental indicators.

The Federal Policy Outcomes measure averages four categories of federal budgeting: percentage of total federal outlays dedicated to human resources; (Non-) defense spending; average income tax rates for the wealthy; percentage of state and local revenues consisting of federal grants-in-aid.

[a] For the integration tests of each series in its levels, the dependent variable of the linear regression equation is the first differences of the series.

[b] This t-statistic, the Dickey–Fuller t, is used to determine whether the series is integrated. For all three series, this statistic fails to reject the null of integration.

[c] The Breusch (1978) and Godfrey (1978) statistic, in these analyses, measures first-order serial correlation among the error terms and is appropriate when a lagged observation of the dependent variable appears on the right-hand side of the regression equation. The test statistic has a chi-squared distribution with one degree of freedom.

[d] A nonsignificant Ljung–Box Q statistic in these tests indicates that the time series has been reduced to white noise.

[e] For the integration test of each series after taking first differences, the dependent variable is the "second differences" (first differences of the first-differenced series) of each series.

[f] This t-statistic, the Dickey–Fuller t, is used to determine whether the series (now represented as changes) is integrated. For all three series, this statistic is highly significant, indicating that the series is stationary in changes and is thus integrated of order 1 in levels.

*$p < .05$.

**$p < .01$, Dickey–Fuller distribution (nonnormal).

to be a function of both previous changes in, and levels of, economic expectations) suggests the appropriateness of an error correction model.

SPECIFYING A DOMESTIC POLICY SENTIMENT ERROR CORRECTION MODEL

Error correction models were first introduced by Davidson and colleagues (1978) in a study of the relationship between income and consumption in the United Kingdom. In general terms, error correction models are appropriate when a priori theory dictates that the dependent variable under analysis exhibit short-term *changes* in response to changes in the independent variable or variables as well as long-term *levels* consistent with those of the independent variable or variables. The notion of a "moving equilibrium" captures the essence of the levels (or long-term) relationship, which is the actual error correction portion of the model. Specifically, error correction models presume that there exists an equilibrium state[14] in which the levels of the time series are typically located vis-à-vis one another. If a shock disturbs this moving equilibrium by forcing the series farther apart (or closer together) than "normal," this equilibrium error is corrected over the long term as the dependent process targets a new level consistent with the equilibrium state.

Due to the natures of economic expectations and domestic policy sentiment, I believe it is appropriate to conceive of the relationship between the two phenomena in terms of error correction. Economic expectations are singular in nature, arising from citizens' evaluations of the U.S. economy. As evaluations change, expectations may respond with little delay. Domestic policy sentiment, on the other hand, arises from the preferences of citizens across scores of distinct issue arenas. I have hypothesized that policy preferences are (in part) driven by economic expectations; but I am assuming that citizens will be more likely to see the direct impact of a changing economic environment on some issues (e.g., federal aid for minorities) faster than others (e.g., gun control). In other words, some policy issues may be more "economically salient" than others. Thus, the shared variance across distinct policy arenas that constitutes domestic policy sentiment is likely to be slow to respond completely to changing economic expectations. The lack of a complete short-term response suggests that equilibrium errors may evolve; and if the relationship I have hypothesized to exist is valid, those errors will be corrected over the long term.

A simple bivariate error correction model may be expressed as follows:

$$\Delta Y_t = \beta_1 \Delta X_{t-1} - \beta_2(Y_{t-1} - \beta_3 X_{t-1} - \gamma) + \varepsilon_t. \qquad (1)$$

Equation 1 relates current changes in Y to both lagged changes in X and the degree to which the lagged levels of Y and X are outside of their equilibrium relationship. The coefficient on the error correction component (in parentheses) measures the rate at which disequilibria are corrected, while γ represents the unknown (and inestimable) constant "spread" between the variables in their equilibrium state. Note that the parameter β_1 captures the short-term relationship between Y and X, while β_3 measures their long-term association.[15]

Consistent with the hypotheses expressed, a single-equation error correction model of domestic policy sentiment was estimated. Current changes in aggregate policy preferences were modeled as a function of short-term changes in, as well as long-term levels of, economic expectations and federal policy outcomes.[16] In addition, four dummy variables representing the Vietnam War, Watergate, the Iran hostage crisis, and the Iran–Contra affair were included (the first two controlling for possible short- and long-term effects, the latter two controlling for short-term effects only).[17] Finally, the time series representing the political component of five-year business expectations (Figure 3) was included, as well. Results of this analysis are presented in Table 2.

As is evident in Table 2, the only phenomena to register statistically significant short-term effects on domestic policy sentiment are the contemporaneous changes in economic expectations and the lagged (by one quarter) changes of federal policy outcomes. This finding demonstrates dramatically the relatively impervious nature of domestic policy preferences, at least regarding short-term effects. Indeed, one must remember that the short-term impact of policy outcomes on policy sentiment is exaggerated, since the policy measure is only "switched on" during a time when such outcomes are presumed to exert their greatest (or only) effect. The error correction coefficient, associated with lagged levels of domestic policy sentiment, indicates that equilibrium errors are corrected at the rate of nearly 70% per quarter. This implies that domestic policy sentiment responds to equilibrium errors relatively quickly, leaving only 30% of the disequilibrating shock present after two quarters, 9% after three, 2.7% after four, and so on. Finally, the error correction model passes several diagnostic tests, including those for serial correlation and hetereoscedasticity.

Table 2

Estimating a Single-Equation Error Correction Model of U.S. Domestic Policy Sentiment

INDEPENDENT VARIABLE	CHANGES IN DOMESTIC POLICY SENTIMENT, OLS COEFFICIENT
Changes in economic expectations	.16* (.07)
Lagged 1 period	
Changes in policy outcomes	−14.34* (5.80)
Changes in Vietnam War dummy	—
Changes in Watergate dummy	—
Changes in Iran hostage crisis dummy	—
Changes in Iran–Contra affair dummy	—
Changes in polit. compon. of long-term bus. expect.	—
Levels of domestic policy sentiment	−.67** (.11)
Levels of economic expectations	.26** (.04)
Levels of policy outcomes	−13.05** (2.86)
Levels of Vietnam War dummy	5.77* (2.10)
Levels of Watergate dummy	—
Constant	40.55** (7.95)

Note: Standard errors are in parentheses. Variables exhibiting nonsignificant coefficients (indicated by dashes) were dropped from the model. R^2 = .45; adjusted R^2 = .41; Standard error of the estimate = 2.81; Durbin–Watson = 1.94. Ljung–Box Q = 18.40 (24 d.f.). (The nonsignificant Ljung–Box Q indicates that the model's residuals have been reduced to white noise.) First-order ARCH = .000. (The nonsignificant first-order autoregressive conditional heteroscedasticity test statistic indicates that the estimated model produces no clustering of large or small residuals.)
$^*p < .05$.
$^{**}p < .01$.

The finding of a contemporaneous (short-term) relationship between domestic policy sentiment and economic expectations raises a reasonable question regarding the proper orientation for an arrow of causality. Generally speaking, one tends to be more comfortable with a temporal *sequence*, such that past events "cause" present events. Yet in this error correction model, current (quarterly) changes in policy sentiment appear to be a function of current (quarterly) changes in economic expectations. This association may lead one to wonder whether the "true" relationship between the two phenomena is one in which changes in policy sentiment cause changes in economic expectations. Such a scenario, however, makes little theoretical sense. It is difficult to imagine the process by which the changing policy preferences of citizens concerning such issues as gun control or federal aid for education might drive their feelings of economic security. Furthermore, it is worth noting that had I been able to aggregate on a monthly (rather than quarterly) basis, a temporal sequence relating past changes in economic expectations to current changes in domestic policy sentiment may have emerged. Finally, I conducted diagnostic tests that suggest economic expectations is exogenous to domestic policy sentiment.[18]

The single-equation error correction model of domestic policy sentiment suggests the following equation:

$$\Delta DPS_t = 40.55 + .16^*\Delta EE_t - 14.34^*\Delta \mathrm{POL}_{t-1} - .67^*DPS_{t-1} + .26^*EE_{t-1} - 13.05^*\mathrm{POL}_{t-1} + 5.77^*\mathrm{VIET}_{t-1} + e_t,$$

where DPS is domestic policy sentiment, POL is federal policy outcomes, EE is economic expectations, and VIET is the Vietnam War dummy. Rewriting this equation in error correction form produces the following:

$$\Delta DPS_t = 40.55 + .16^*\Delta EE_t - 14.34^*\Delta \mathrm{POL}_{t-1} - .67(DPS_{t-1} - .39^*EE_{t-1} + 19.48^*\mathrm{POL}_{t-1} - 8.61^*\mathrm{VIET}_{t-1}) + e_t,$$

where the portion of the equation in parentheses represents the error correction component of domestic policy sentiment. According to the model, domestic policy sentiment will be in an equilibrium relationship vis-à-vis economic expectations, federal policy outcomes, and the (post-)Vietnam political environment when this expression is equal to zero. Any shock to this equilibrium relationship will be corrected by changes in domestic policy sentiment at the rate of 67% per quarter, beginning one quarter after the shock is experienced.

Interpreting the coefficients in the error correction model is a straightforward matter. Starting in an equilibrium state and holding all else constant, assume that economic expectations improves by five points (roughly an 8% increase, based on the variance of the series during the time period under observation) and that such a change in outlook is maintained. The short-term impact of this change will be a contemporaneous (i.e., same-quarter) liberal shift in domestic policy sentiment of .8 points, or approximately 3%. Since domestic policy sentiment and economic expectations maintain an equilibrium relationship when the lagged level of policy sentiment is equal to slightly more than one-third of the lagged level of expectations,[19] the resulting disequilibrium will be characterized by policy preferences that are too conservative (i.e., too low on a scale on which higher scores indicate more liberal values). In order to correct this disequilibrium, domestic policy sentiment will ultimately shift in a liberal direction (increase) by an additional 1.95 points, or slightly less than 7%. This reequilibration will take place over a number of quarters, with the largest portion (67%) coming one quarter after the initial shock. Thus, domestic policy sentiment will increase (move in a liberal direction) by 1.31 points one quarter after the jump in economic expectations, then .43 points (or 67% of the remaining disequilibrium, .64 points), then .14 points, and so on. The total short- and long-term impact of a five-point change in economic expectations on domestic policy sentiment, then, will be a shift in policy sentiment of 2.75 points, realized over approximately five quarters.

CONCLUSION

This analysis suggests that the meandering course that public opinion (conceived of as domestic policy sentiment) follows through time is not random but, rather, driven by important economic and political phenomena. It has been demonstrated that once the effect of policy outcomes in the 1970s and 1980s is controlled, domestic policy sentiment responds strongly to changes in economic expectations. As the American public views the future economy pessimistically, conservative domestic policies gain greater favor. Conversely, an optimistic economic outlook results in greater support for liberal domestic policies.

This has important implications for both students and practitioners of American politics. From the perspective of policymakers and citizen activists, the knowledge that the public's willingness to support certain policy initiatives is directly related to perceptions of the economy offers considerable strategic guidance. Once might conclude, for example, that efforts at initiating liberal policies are ill advised during troubled economic times. By the same token, supporters of conservative domestic policies may benefit from the assumption that the public is more likely to accept their initiatives in the midst of an economic downturn. The same principles apply equally well during good economic times: supporters of liberal policies are more likely to encounter an acquiescent public, whereas conservative policymakers and activists might be better off waiting for another day. Economic expectations, then, play a critical role in the opening and closing of the "policy windows" through which the makers and advocates of policy initiatives must move (Kingdon 1984).

For students of American politics, this study suggests that it is no longer possible to treat public opinion as an exogenous phenomenon. Many theorists and empiricists have long assumed that policy preferences, albeit of critical importance to the policy process, reside *outside,* rather than inside, the system of policymaking. My analysis sits in direct contrast: both economic expectations (and by implication the economic policies enacted by the government) and federal public policymaking are capable of changing the public's domestic policy sentiment. This knowledge may have a profound effect on how analysts of American public opinion and policy approach their subjects.

While this effort has made inroads into the study of policy preferences, it is by no means the final word on the subject. One would be hard pressed to believe that economic expectations and federal policy making *alone* affect domestic policy sentiment. Future analyses will undoubtedly feature better-specified models, as well as superior operationalizations of both economic expectations and domestic policy sentiment. Nonetheless, in light of the present findings it is difficult to conceive of any model of policy sentiment that does not allow for the powerful effects of economic expectations.

NOTES

I thank Jim Stimson and Thom Yantek for their advice and helpful comments on earlier drafts of this paper.

1. Some readers may note that a hypothesis linking greater economic security with an increased willingness to support liberal domestic policies echoes the fundamental idea of Inglehart (1971). Borrowing from Maslow's (1970) notion of a needs hierarchy, Inglehart presumes that "individuals pursue various goals in hierarchical order—giving maximum attention to the things they sense to be the most important unsatisfied needs at a given time" (1971, 991). Inglehart then proceeds to develop a generational model in an effort to demonstrate that age cohorts raised in an environment of relative economic security will be less concerned with economic issues in favor of a more "postmaterialist" agenda. While this work shares the starting point of Inglehart—as well as that of Dalton (1988), Flanagan (1987), and Sirgy (1986)—the destination is altogether different: rather than *uni*directional, *inter*generational evolution, I anticipate a *cyclical* (in a loose sense of the word, implying only back-and-forth movement), *intra*generational dynamic. When faced with a perceived downturn in the economy, the same generation that some years back embraced a liberal policy agenda during good economic times is likely to swing back to the ideological right.

2. For a complete description of this process, see Stimson 1991a, chap. 3. Note, however, that I here use a slightly different algorithm. In addition to working backward through time from the most recent quarter (1988:1), the new algorithm also starts with the earliest time period (1968:2) and proceeds to the most recent, averaging the two resulting time series before producing an iterated solution.

3. Stimson's (1991a) analysis deals exclusively with annual data, covering the time period 1956–89. Since the algorithm used to create the time series depends upon the presence of multiple items per time period, my decision to aggregate at the level of quarters forces me to restrict my analysis to the years for which we have the most public opinion data (1968:2–88:1). Repeated attempts to push the series back in time failed due to a lack of sufficient observations prior to 1968. And even for the years under analysis, it was necessary to add 29 interpolated administrations of 24 different survey items. Failing to do so results in a time series considerably more noisy than the one shown in Figure 1. Note that the addition of the interpolated observations accounts for 3.7% of the total data base; this is roughly equivalent to 3 interpolated values in a time series of 80 observations.

4. For an account of these interesting relationships, see Stimson, 1991a.

5. As it turns out, such a distinction between long- and short-term economic evaluations is, at least operationally, rather trivial. Included among the ICS survey items is one that asks respondents to assess short-term (one-year) future business conditions. The product–moment correlation between the resulting time series and the long-term business conditions series is .95. Nonetheless, I stick to my guns on purely theoretical grounds: a survey item asking individuals to think long-term is more likely to capture what I seek.

6. Although political scientists have in the past restricted themselves to models of consumer sentiment (and other phenomena) that presume that lagging or coincidental measures of the economy are most appropriate, it is worth noting that the U.S. government has, since 1989, included a measure of consumer confidence—the Conference Board's Index of Consumer Expectations—among its leading indicators of the economy. Indeed, it makes little sense to presume a prospectively oriented public and then proceed to develop a model that predicts current consumer sentiment on the basis of last quarter's economic conditions. And as is demonstrated by MacKuen, Erikson, and Stimson (1991), consumer sentiment appears to respond not to current or lagged economic conditions but, rather to *leading* indicators of the economy. Nevertheless, the distinction between leading, coincidental, and lagging models is limited to the realm of theory. The time series of residuals resulting from all three approaches correlate with one another at the average rate of .90. Thus, the choice of modeling strategy is statistically irrelevant.

7. This modeling strategy depends upon zero covariance between the economic and political components of business expectations. If they were to covary, the estimated effects of economic conditions on business expectations as captured via linear regression would be biased; and it follows that the predicted values (i.e, economic expectations) based on the economic measures will be incorrect. To test for covariance, I regressed business expectations on four measures of the objective economy *and* the time series of the University of Michigan's Survey of Consumer Attitudes, the latter serving as a surrogate for the political component. I then generated predicted values based only on the coefficients associated with the objective measures. The resulting series correlates with one generated by only the economic measures at the rate of .96, suggesting that covariance between the political and economic components is not present.

8. Although the decision to aggregate quarterly restricts this analysis to the years 1968–88, both time series may be aggreated annually back to 1956 for the purpose of determining whether the pattern exhibited in Figure 4 is maintained. Having done so, I am pleased to report that the product–moment correlation between the annual series (1956–88) is .81. The correlation between the quarterly series presented in Figure 4 is .69.

9. The mean intercorrelation coefficient for the four time series is .53.

10. In the analysis, this time series is scored as a "step" function, taking a new value only at the outset of a new fiscal year. This method of operationalization is preferred due to its greater similarity with the actual (annual) budgetary process. Nonetheless, I ran the complete analysis with an interpolated measure of federal expenditures; and the differences in coefficients were nonsignificant.

11. My primary motivation here is to improve the estimation of the relationship between economic expectations and policy sentiment. Such a relationship is not strengthened by the inclusion of the policy measure. Indeed, quite the contrary is true. The coefficient on economic expectations is cut by one-third in a model that includes the policy outcomes time series, but the overall fit of the model is improved substantially.

12. Theorizing about, and estimating with some precision, the degree to which these events affected domestic policy sentiment is beyond my scope here. In the analysis that follows, I seek only to control for the possible effects that these events may have registered.

13. See MacKinnon, 1990 for a complete discussion of the procedures involved in integration diagnostics.

14. *Equilibrium* is used here in a less formal sense than is typical in economic literature, implying only that the phenomena maintaining an equilibrium relationship are loosely tied to one another.

15. For further reading on error correction models, see Beck n.d.; Engle and Granger 1987; Hall and Henry 1988, Durr n.d., and Ostrom and Smith n.d.

16. In an effort to specify correctly the lag lengths for short-term effects, I used vector autoregressions as diagnostic tools, following the procedures outlined by Engle and Granger (1987).

17. The inclusion of dummy variables in error correction models warrants more care than is typically exercised (see Durr n.d.). Specifically, such variables should not be switched on and off abruptly (i.e., jumping from zero to one and back) but, rather, should mimic the events represented, growing and/or decaying exponentially. Furthermore, dummy variables representing phenomena presumed to exercise a long-term (reequilibrating) effect should never be switched off. For example, to the extent that Vietnam affected policy preferences *in the long term*, it is assumed that "post-Vietnam" is "post-Vietnam"—whether 1975 or 1985. In other words, the long-term equilibrium relationship anticipated is that between policy preferences and a political environment that includes Vietnam in its past. To wit, the four dummy variables included in the estimation of the error correction model of domestic policy sentiment are

1. Vietnam, growing exponentially to a peak value of one at 1970:2 and maintaining that value throughout. This corresponds to the 4 May 1970 shootings at Kent State University, an event that brought the war home. (Those more comfortable with an earlier starting date are reminded that since my analysis begins with the second quarter of 1968, a dummy variable switched to one by that point and permitted to maintain that value throughout would be perfectly collinear with the constant term.)
2. Watergate, growing exponentially to a peak value of one at 1974:3 (corresponding to Nixon's resignation in August of 1974) and maintaining that value throughout.
3. The Iran hostage crisis, jumping from zero to one at 1979:4 (corresponding to the taking of hostages in November of 1979) and returning to zero exponentially, starting at 1981:1 (corresponding with the release of the hostages).
4. Iran–Contra, jumping from zero to one at 1986:2 (corresponding to the November 1986 Ronald Reagan press conference) and remaining at one, since the analysis only extends to 1988:1.

18. If economic expectations were driven in the short term by domestic policy sentiment (whether in the form of feedback or some other processes beyond my imagination), a block of policy sentiment (expressed in differences) would be significant in an unrestricted vector autoregression of current changes in economic expectations. Evidence of a long-term association would be a significant coefficient on the lagged levels of economic expectations in the same. Neither scenario proves to be the case.

19. Plus some unknown constant, which is presumed to exist but cannot be estimated.

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Robert H. Durr is Assistant Professor of Political Science, Washington University in St. Louis, St. Louis, MO 63130.

CONTROVERSY

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POLARITY AND INTERNATIONAL STABILITY

In his article in the June 1991 issue of this Review, *Ted Hopf challenged the argument that bipolar systems are inherently more stable than multipolar configurations of power. He reported that the international situation in sixteenth century Europe became only marginally more stable with a shift from a multipolar to a bipolar system. He argued for attention to the offensive-defensive balance, rather than systemic polarity. Manus Midlarsky accounts for Hopf's findings, and for evidence of multipolarity and increased conflict in the early seventeenth and twentieth centuries, by proposing a relationship between polarity and war that is contingent on scarcity of desired international resources. Midlarsky argues for more attention to contingent relationships generally and draws implications for current and future probabilities of conflict in a multipolar world. Hopf responds by pointing out the need for further development of Midlarsky's analysis, the possible compatibility between Midlarsky's formulation and his own focus on the offensive-defensive balance, and the desirability of a productive unity of the two research programs.*

COMMENT

Ted Hopf (1991) is to be commended for his use of systematic methods to examine a hypothesized relationship between polarity and international stability. His use of historical materials, statistics, and dimensions of comparison all appear to be satisfactory. I accept his conclusion concerning the absence of a relationship between polarity and stability for his time period of investigation. I also accept his measurement of international stability by various war indicators. In fact, for purposes of this critique, I will assume that all is well within the confines of his study, as far as it goes. The basic problem with his article is that it does not go far enough. That difficulty is compounded by the implications that Hopf draws for a benign multipolar future that simply cannot be accepted at face value, given the confines of his analysis.

My purpose is twofold. First, I will demonstrate the problems of inference incurred by the limited theoretical and empirical domain of Hopf's study. Second, by widening that domain, I seek to introduce an entire literature that on the face of it, may not appear to be especially relevant to the issue of polarity and international stability but, in fact, may be critical to that relationship.

Hopf purports to demonstrate the irrelevance of polarity to stability by comparing two adjacent time periods in the European system, one multipolar (1495–1521) and the other bipolar (1521–1559). His finding of virtually no observable difference between the two periods in various indicators of warfare, contrary to Waltz's (1979) arguments, is taken as confirmation of the irrelevance of polarity to international stability. He further states that this finding implies a relatively peaceful multipolar future. Abandoning the polarity variable because of its seeming failure to predict any difference in warlike behavior between the two periods, Hopf turns primarily to the offense–defense balance and finds greater explanatory power in this variable. But was he justified in that hasty abandonment?

A CONTINGENT RELATIONSHIP BETWEEN POLARITY AND STABILITY

Hopf presents the relationship between polarity and stability as if it were unconditional, that is, as if there are no variables upon which that relationship is contingent. In that view, finding no difference between a multipolar and bipolar period in regard to war dooms the hypothesis to perdition or, in our lexicon, *disconfirmation*. But if there are studies finding that this relationship—at least analytically, if not empirically—is indeed contingent on a third variable, then one cannot so readily reject the hypothesized relationship. And this, in effect, was found in my studies of the relationship between polarity and systemic stability (Midlarsky 1988, chaps. 3 and 4; Midlarsky 1989). Instead of an unconditional relationship, the association between polarity and international stability was found to be contingent on the scarcity of international desiderata. If such desiderata are abundant in the international system, then there is not much, if any, difference between the stability of bipolar and multipolar systems. Under scarcity conditions, however, there is a great difference between the two, with the multipolar condition tending toward greater instability, the greater the scarcity of desired commodities. Bipolarity is not subject to such constraint, experiencing stability whether under conditions of scarcity or abundance.

This contingent relationship between polarity and stability was discovered using a probabilistic relationship between scarcity and inequality. In multipolar systems, the greater the scarcity of desiderata, the greater the inequality between most- and least-favored actors. This inequality can then lead to envy by the less-favored actors and the rupture or formation of alliance bonds targeting as "enemy" the most favored international actor. Bipolar systems experience equality between the two poles regardless of scarcity conditions. Possibilities for direct negotiation and fine-tuning between the two blocs add directly to the equalization process.

Now how does this contingent relationship play out in the context of Hopf's study? One must first examine the overall environment of scarcity or abundance in his chosen time periods. This is especially important, of course, for the multipolar condition, perhaps subject to scarcity-induced inequality and instability. Using sources cited by Hopf, alas, there is no reason to expect that the two periods—one bipolar and the other multipolar—should behave differently with regard to indicators of stability. Both periods are part of an overall period of expansion in the European and world systems that Braudel dates at least from 1470, if not 1450 (1972–73, vol. 2, 893). The end of this period of strong expansion and prosperity coincided approximately with the ending of the sixteenth century if not later in the early seventeenth century, as economic processes slowed and, in some instances, actually reversed course. Thus, Hopf's overall time period of 1495–1559 is well within this period of abundance. Bipolar and multipolar systems should exhibit no significant differences regarding stability under this condition of abundance—as, in fact, they do not in his analysis.

Consider this period of expansion in more detail. The New World was discovered in 1492, engendering a vast new set of opportunities (especially for plunder) to be exploited by Spain, Portugal, and England within the context of an already-burgeoning prosperity. France was in the process of state building after the successful expulsion of the English from virtually all of their French possessions during the fifteenth century. Successful rationalization of tax collection and a general increase in government revenue characterized this period. Braudel, for example, quotes one Gilles Caster, who tells us that "energy returned to Toulouse in the years 1460–1470" and also writes that this city experienced a full century (1460–1560) of prosperity (1972–73, vol. 2, 895).

Concerning Venice, one of the poles of the multipolar system identified by Hopf, Braudel writes:

> In Venice, which I have personally studied in some detail, I am impressed, for example, by the scale of public building and decoration in the city after 1450: the replacement of the wooden bridges over the canals by stone bridges, the digging of the great wall near the church of Santa Maria di Brolio in August, 1445, the construction in May, 1459 of the new loggia *in loco Rivoalto*, where the weavers' shops were demolished to make way for the extension to the Doges' Palace. "The city increases in beauty every day" notes a document of 1494, "let us hope people will respect its beauties." (1972–73, vol. 2, 894–95).

Lane concurs: "The general economic growth of the sixteenth century affected Venice most strikingly by expanding the markets for her manufactures" (1973, 309). The Ottoman Empire, too, was expanding during this period. Concerning its effects on Venice, Lane writes: "The power of a sultan ruling from the walls of Vienna to Arabia and to the border of Morocco turned Constantinople into a flourishing city again. Its population rose from less then 100,000 in 1453 to about 700,000 in 1580, and the wealth of the sultan's court made it a major market for Venetian merchandise" (p. 304). Only in central Europe was the prosperity enjoyed by the elite mitigated by a rising peasant wrath that would have strong implications beyond this period (Huppert 1986, 82–88).

Braudel summarizes the economic activity of the sixteenth century: "A slow but powerful upsurge favoured the advance of the material economy and of everything dependent on it. It was the secret of the fundamental healthiness of the economy. 'In the sixteenth century' Earl J. Hamilton once told me, 'every wound heals over' " (1972–73, vol. 2, 893). In consequence of this fundamental health, "the constant pressure of the mounting secular trend clearly appears to have encouraged the growth first of territorial states and then of empires" (pp. 895–96). This is in marked contrast to later periods, such as the first several decades of the seventeenth century and those immediately preceding the onset of World War I, as I shall demonstrate shortly.

At one level, then, given this state of increasing abundance and expansion, Hopf's finding concerning polarity can be understood as a confirmation of the contingent relationship between polarity and stability. The theory predicts little if any difference in warlike behavior between bipolarity and multipolarity under conditions of abundance and this is precisely what Hopf found. At another level, however, his finding can be viewed as a confirmation of the obvious; for without substantial variation on the contingent variable of concern—namely, the extent of scarcity or abundance—one simply would not expect any difference between his two time periods of analysis. Another period is required for comparative purposes—one subject to contractions or at least one in which significant scarcities appear. If such a multipolar period exists in relative temporal proximity to the others, then it can contribute variation and expand the empirical test.

One can choose an earlier multipolar time period demonstrating significant scarcites for comparative purposes, but information availability is circumscribed prior to 1495, the beginning of Hopf's period of analysis. Among time periods subsequent to his—that is, after 1559—the first serious choice is presented by the beginning decades of the seventeenth century, as the first subsequent period during which scarcities were evident. The expansions of the preceding century had slowed and contractions now began to appear. Again, Braudel informs us of general conditions in the Mediterranean region but which had echoes throughout the whole of Europe:

> There is no doubt that during the fifteenth century, when the population was small, new land became available to the peasants of the Mediterranean. It was a time of expansion, or rather of recovery of a former prosperity, that of the thirteenth century. An agricultural revo-

> lution undoubtedly preceded and supported all the expansionist movements of the sixteenth century as Ruggiero Romano has rightly argued. But in the end this forward movement was brought to a halt by the very inelasticity of agriculture, under the same conditions as in the thirteenth century. The reclaimed land often gave an inferior yield. The number of mouths to feed was increasing more quickly than the resources and the logic of later Malthusian arguments was already visible. . . . The basic explanation must lie in the inelasticity of agricultural production. It had reached its ceiling and the result of this impasse was to be the "refeudalization" of the seventeenth century, an agricultural revolution in reverse. (1972–73, vol. 1, 426–27)

Regarding central Europe of the early seventeenth century, Wedgewood, among others, points to population growth and the agricultural crises of that period (1972, 28). In England, too, such processes led to a class of unskilled urban poor and increases in the work-dependent working population (Wrightson 1982, 138–40). Other references to population growth and its economic consequences can be found in Briggs and Slack (Briggs 1983, 122; Slack 1985, 181–82). In short, the situation that faced Europe in the early seventeenth century was radically different from that of the preceding century. Instead of an expanding period of the formation of states, empires, and thriving economies, Europe faced a set of constraints that prevented such expansion and indeed, in certain instances, actually led to contractions.

A consequence of this overall process was the "deeply felt crisis of 1619–1623" (Braudel 1972–73, vol. 2, 893). Although there were earlier downturns in the sixteenth century, Braudel singles out no other in this fashion for its severity. Additionally, for Central Europe, no other downturn has the magnitude shown in Braudel's graph of money devaluations, especially for Austria, initially and subsequently, the single most heavily involved great power in the Thirty Years' War (1972–73, vol. 1, 528). This was one of the economic crises that led Braudel to the general observation, "In fair weather, family quarrels came to the fore; when times were bad, war was declared against the Infidel" (1972–73, vol. 2, 898). Wars of the latter type reflecting religious or ideological differences during this period (e.g., the Austro–Turkish War, the Thirty Years' War) were far more intense and enduring than those among states with like-minded world views.

Only in its multipolar status was this period similar to its sixteenth-century counterparts. The Ottoman Empire, the Habsburgs (Austria and Spain), Denmark, the Catholic League, the Protestant Union, the United Provinces (Netherlands), England, France, and Sweden all were polar actors during this period.

My understanding of polarity in this context is straightforward. To qualify as a pole of the system, any state (or coalition of states) must be able to mount an effective—in the sense of having a reasonable probability of success—military engagement with any other state (or coalition) of the system. By this standard, the early seventeenth century is multipolar in the extreme. For example, the Catholic League, a coalition of smaller Catholic states with Bavaria as its mainstay, was able to field an army of 30,000 under Tilly early in the Thirty Years' War in 1620 (Parker 1984, 61). This army was quite successful against its Protestant opponents (army sizes of this magnitude were typical of the time period) (Hale 1985, 63). Later in the seventeenth century and during the Thirty Years' War, army sizes increased so that only the larger and most deeply involved powers such as Austria, France, and Sweden could compete effectively. At their heights of activity in the war, Sweden had approximately 100,000 men in the field; France, 125,000; and Austria, 151,000 (Parker 1984, 100, 133, 147). Even by this more exacting standard—effectively controlling for size, extractive capacity, and resolve as the result of intense involvement in the conflict—the system still is multipolar but not to the same extent as earlier. Further descriptions can be found in Langer 1980, Pagès 1970, Polisensky 1971 and 1978, Rabb 1972 and 1975, Steinberg 1966, and Wedgwood 1938.

Had Hopf used the early seventeenth-century multipolar period reflecting significant scarcities to compare with his previous two—one multipolar and the other bipolar—he might have reached a radically different conclusion. The Thirty Years' War of 1618–48 (beginning its intense phase in 1621) is clearly relevant. The number of battle deaths and duration associated with this war alone would have established a clear distinction between multipolarity under scarcity versus the same system under relative abundance. Of central importance here, the much greater instability of multipolarity under scarcity when compared with bipolarity would also have become evident.

Most interesting about the instability of multipolarity under scarcity conditions is that one can see the dangers without necessarily having to derive the relationship formally, as I did. The period preceding World War I is a case in point. The international politics of that period were driven very much by the acquisition of colonies (a distinctly limited resource), either in the form of direct rule by Great Britain, France, and Germany or in the form of political satellites to be heavily influenced by Austria–Hungary or Russia. As the Ottoman Empire began to come apart, especially in Europe, the emerging independent states either found themselves under Austro–Hungarian or Russian tutelage. Indeed, so important does Kennan (1979, 1984) deem this process that he attributes the breakdown of the *Dreikaiserbund* (uniting Austria–Hungary, Germany, and Russia) and the consequent formation of the Franco–Russian alliance to Russian failure to acquire such satellites, especially Bulgaria in 1885. The Franco–Russian alliance, of course, was the building block of the Triple Entente that was to oppose the Dual Alliance in war in 1914. Great Britain's conflicts with France on the one hand and Germany on the other over colonial acquisitions at Algeciras (1906), the "Casablanca Incident" (1908), Agadir (1911), and the Berlin–Baghdad railway, of course, fueled conflict matters in the West (Thomson 1966, 483–87).

Here the dominant scarcity appeared in the availability of colonies or political satellites that diminished rapidly toward the end of the nineteenth century and contributed strongly to the conflict patterns in both Western and Eastern Europe prior to World War I. Indeed, this halt in empire formation in the early twentieth century bears strong similarities with the virtual termination of such processes in early seventeenth century continental Europe. While most scholars treat the twentieth century pre–World War I period as one of economic expansion (see Goldstein 1988, 67), it was precisely during the preceding period of economic downturn (circa 1893) that the basic alliance structure of France and Russia versus Austria–Hungary and Germany was consummated. The upturn that occurred during the early twentieth century was apparently insufficient to overcome the already entrenched hostilities manifested by these two opposing coalitions and their intensifying conflicts over colonies and satellites that would eventually embroil Great Britain, as well.

One may also interpret the onset of World War II as having been conditioned strongly by scarcity in the form of the economic depression of 1929 and the various national and international political responses to it in that multipolar environment. The rise of authoritarian polities and consequent patterns of alliance formation can be attributed, in part, to the dominant scarcities of employment, trade, and other opportunities during that period. Uncertainties in the identification of politically relevant scarcities will be treated shortly.

A related perspective is suggested by the recent work of Saperstein (1991). Using a model of the arms race, he demonstrated a substantial increase in instability upon the transformation of a bipolar system into a tripolar one. Small perturbations of the system led to large changes in the tripolar case but not in the bipolar. Of greatest interest here is the finding that a weak coupling of the third pole to the first two does not yield much change; but the stronger the coupling of the third pole to the competitive international structure, the greater the descent of the system into chaotic behavior.

A plausible interpretation of these findings is that as additional major powers compete for ever-scarcer resources, the stronger coupling of the new actors into the competitive international environment is transformed into new armaments competitions that fuel the instability of the multipolar system. The period preceding the Thirty Years' War—and especially that prior to World War I—can be characterized in this fashion. In this interpretation, arms competitions are not treated in isolation from polarity and the scarcity or abundance of desiderata (as Hopf did in his separate treatments of polarity and the offense–defense balance) but as part of the same competitive process that has several different manifestations of that competition.

Hopf certainly cannot be faulted for having been unaware of Saperstein's work, given its publication date. My purpose in discussing it is not only to highlight those of Saperstein's findings that I believe to be important but to suggest strategies of inquiry that may be more productive than Hopf's examination of one hypothesized bivariate relationship between polarity and war, its rejection, and separate consideration of other possibilities. More on that later.

DIMENSIONS OF SCARCITY AND ABUNDANCE

Turning now to the second of my purposes, the introduction of an important cognate—but not transparently relevant—literature, the specific scarcity contributing to political conflict is a matter of importance. At any given historical time, one may identify (but not with certainty) some major dimensions of scarcity or abundance. And this, in fact, is one of the axes of the vigorous debate between Paul Ehrlich and Julian Simon. Ehrlich sees population growth and consequent pressure on resources as ushering in a period of increasing scarcity of valued commodities. Simon, on the other hand, views increased population as contributing to greater output and technological innovation in generating new resources. The various issues are laid out in the debates in *Science* (Singer et al. 1980; Simon 1980), *Public Opinion* (Hardin and Simon 1982) and the *Social Science Quarterly* (Ehrlich 1981a, 1981b; Simon 1981). The New York Times Magazine featured an article on a bet between Ehrlich and Simon concerning the rise or fall in the price of certain metals (Tierney 1990), a bet that Simon recently won. Basic views of both writers can be found in Ehrlich, Ehrlich, and Holdren 1977 and Simon 1990.

Ehrlich's concerns have been with processes such as desertification, loss of agricultural land to cities, environmental disruptions (e.g., pollution), and too much energy use. To him, these are symptomatic of a general environmental degradation that can only get worse as population increases and puts increasing pressure on these increasingly scarce resources. Simon's response has been that problems of this sort, although serious, have generated human responses of great ingenuity and discovery so that society winds up in a more advantageous position than before the problems arose.

It is clear that much of contemporary technical and social thought sides with Ehrlich. I personally veer toward Ehrlich's interpretation, especially in light of the uniquely social consequences of scarcity, such as inequality, and the consequences of scarcity in the form of the American urban disorders of the 1960s as an early response to scarcity-induced inequality (Midlarsky 1982, n.d.). The current decline of the American middle class and the urban environment, as well as hard-fought battles with the Japanese and Europeans for domestic and international markets is further evidence for growing economic scarcity as a general societal condition in the United States.

Yet there is much to be said for Simon's position. Although worldwide population growth is a reality, presumably exacerbating the scarcity condition, he won the 10-year-old bet with Ehrlich concerning the prices of metals (Tierney 1990). Instead of rising, as Ehrlich predicted, they declined, indicating new resources and technological innovation to counter the unremitting population pressure successfully.

Whether Ehrlich or Simon is correct, of course, bears heavily on the contingent relationship between polarity and war. If Simon is correct, then multipolarity under conditions of abundance and bipolarity should behave in a similarly benign fashion, thus making Hopf's implications concerning a peaceful multipolar future essentially correct. However, if Ehrlich is right, then multipolarity under conditions of increasing worldwide scarcity could be perilous. The battle for markets and other axes of conflict could set up new alliance arrangements to counter economic "enemies." Multipolarity under these conditions could yield extremely dangerous international conditions. Thus, my response to Hopf's implications for a nonthreatening multipolar future, is that it all depends. In the final analysis, this should not be surprising.

STRATEGIES OF INQUIRY

There may be a larger epistomological truth underlying all of this. Relationships between two nontautologous societal variables are seldom, if ever, absolute; they are almost always contingent on third, fourth, and perhaps even additional valuables. These contingent relationships are most likely to reveal the structures of political life; for they indicate the domains (theoretical, spatial, temporal) within which bivariate relationships hold and by implication, of course, where they do not.

Parallel to this observation is one concerning the etiology of large-scale international conflict that can distinguish one time period from another. It is often the *combination* of two or more simultaneous processes that sets the stage for major and enduring political violence. Skocpol (1979), for example, found that it was the simultaneous presence of conflict within the societal elite and between that elite and the peasantry that increased the likelihood of mass revolution. Similarly, I found that it was the conjunction of major power conflicts alone (essentially in Western Europe) and those between major powers involving at least one minor power (largely in the Balkans) that set the stage for World War I (Midlarsky 1988, chap. 5). Each of these conflict sets alone was found to be insufficient to initiate serious instability. Only jointly were they found to be associated with the onset of systemic war.

Obviously, the absence of one or more of these conditions can increase the likelihood of a peaceful period. Although at the time of this writing there is intense political violence occurring in the former Yugoslavia, it is extraordinarily unlikely that it will intersect with a multipolar conflict system in Western Europe, since as yet, one does not exist and is unlikely to come into being in the foreseeable future. It is the absence of a multipolar Europe that ensures the absence of widespread European warfare at this time, despite the rumblings in the Balkans. (Indeed, whatever peace was achieved at the outset apparently resulted from the concerted efforts of European Community representatives.) Thus, the absence of a multipolar system in the post–World War II period and its replacement by a bipolar one *combined* with nuclear weapons in a defense-dominant system could have contributed to recent international stability. The possible conjunction of two or more necessary conditions for war or peace, therefore, should not be overlooked; neither should we ignore the contingent relationships that can define our domains of inquiry.

Manus I. Midlarsky

Rutgers University

RESPONSE

I have four general points. First, Midlarsky attributes more ambitious claims to my argument than I intended to make. Second, while I find Midlarsky's suggestions for improvement to be a provocative line of reasoning, I think its ultimate fruitfulness as an avenue of research will depend on clearing up several logical ambiguities and on a great deal more empirical investigation. Third, I think it quite likely that one of the elements of my offense–defense balance, namely, the *cumulative nature of power resources*, nicely captures the essence of Midlarsky's suggested alternative. Finally, I see no obstacle at all to a most productive marriage between Midlarsky's research program and my own.

VALIDATION NO, INVALIDATION YES

In part, Midlarsky is responding to claims I have not made for my theoretical alternative to Waltz. I did suggest that my own conceptualization of the offense–defense balance should be explored as an alternative to polarity. I still maintain that position on the grounds that polarity failed its test in sixteenth-century Europe and that the predictions of the offense–defense balance were consistent with the empirical record for the period. But I did not suggest that this one rather successful test was any kind of *general* validation of the theory. Indeed, on the very first page, I cautioned the reader that "solid support" for my theory still awaited "definitive demonstration" (p. 475). I stressed this difference between disconfirming Waltz and confirming my alternative and suggested a list of historical cases of bipolar and multipolar systems in which the competitors should be tested (p. 487, n. 14). Having just criticized Waltz for relying on only the single case of post–World War II bipolarity to ground his claims, I could hardly hold myself up to weaker standards of evidence.

Midlarsky is right, however, to believe that I think my empirical test has given polarity a failing grade. Polarity's standing as a causal predictor of great-power war has been seriously undermined. Multipolar Europe was no more unstable than bipolar Europe from 1495–1559. This empirical finding is inconsistent with Waltz's theory. Moreover, Waltz's argument that bipolarity kept the peace during the Cold War is confounded by the defense dominance provided by nuclear weapons.

SCARCELY AN ABUNDANT ALTERNATIVE

Midlarsky argues that the unimportance of polarity in sixteenth-century Europe was due to its being an era of abundance. Multipolar systems are marked by war only in eras of scarcity, so that it is only to be expected that one would see no change from 1495 to 1559, despite the change from multi- to bipolarity in 1521. If this is correct, then the offense–defense balance is irrelevant to the level of stability in international systems; and polarity is relevant, in combination with "international desiderata." For reasons logical, methodological, and empirical, I believe Midlarsky's alternative to be flawed.

First of all, Midlarsky leaves international scarcity and abundance ill defined. At various places in his essay, he lists state building, "increase in government revenue," "the replacement of the wooden bridges over the canals by stone bridges," Venice's increasing beauty, imperial expansion, employment, and trade as indicators of this concept. Obviously, this variable needs to be tightened up and clarified. Unless otherwise noted, I shall assume that Midlarsky means average level of economic growth in the international system. (Even this seemingly simple operationalization is problematic. Why should we care about everyone's economic growth? Why not just the great powers? Or an individual great power? Or great powers' economic growth rates *relative* to each other? Much more theoretical guidance is required here before this "alternative theory" can be considered adequately specified.)

Second, there are areas of clear logical indeterminacy in many of the hypotheses generated from this alternative theory. For example, Midlarsky uses "population growth" as evidence of scarcity. Why should this be? In periods where manual labor contributes to productivity growth, one could imagine population growth being positively associated with abundance. Where economies cannot absorb such demographic increases, we would term it a sign of scarcity. After all, population grew rapidly from 1495 to 1559; yet Midlarsky calls this a period of abundance. If rate of population growth is just one component in a more complex indicator of scarcity/abundance, then we need to have this explicitly specified in the theory. Moreover, we should expect that different states will prove more and less able to take advantage of factors of abundance and so will behave differently in response to conditions of plenty and scarcity. This consideration, too, must be taken into account when specifying the elements of this alternative theory.

It is also hard to understand why imperial expansion is a sign of plenty. If plenty is associated with peace, how does imperial expansion, also an associate of plenty, play it itself out without threatening peace among the great-power competitors? I can easily conceive of good arguments for both cases and so am inclined to think of the logical implications of this variable as indeterminate. Moreover, it is hard to accept the logic underpinning the idea that scarcity is more likely to cause instability under multipolarity than bipolarity. When faced by scarcity, it is no longer possible for a state to defend itself through "internal balancing" (i.e., exploitation of economic resources at home). In a bipolar world, there is only one alternative, external balancing through conquest. Under multipolarity, however, there are *two* alternatives: conquest and alliance formation. Therefore, there is a lower, not higher, probability of instability under multipolarity than bipolarity.[1]

Inadequate specification of his alternative is manifest in Midlarsky's identifying imperial expansion as evidence of plenty in the sixteenth century; but he calls the very same variable an indicator of *scarcity* before World War I! Indeed, the race for colonies in the Americas, which he argues is an indicator of plenty in the sixteenth century, becomes an indicator of scarcity in the run-up to World War I when it takes place in Africa. Moreover, in Midlarsky's empirical treatment of the latter period, he ignores some indicators of plenty he had previously identified, namely, economic growth, population growth, and expanding government revenues. Each of these would have shown abundance in the international system prior to an event of epic instability.

I have no argument with Midlarsky's empirical evidence for instability in the early seventeenth century, but he considers no alternative explanations. While the European world economy was contracting, the ability to wage offensive warfare, given new advances in field and siege artillery, could have been progressing simultaneously. This would have the same effect as scarcity and should be investigated.

I agree that the early seventeenth century should be examined. Indeed, I advocate testing the offense–defense balance in many other historical periods (as suggested in my article). As far as Midlarsky's period is concerned, we need figures on instability in his period to compare to mine. While Midlarsky claims that the many wars in the seventeenth century are the product of multipolarity plus scarcity, he does not show that the instability at that time was any greater than in the bipolar period from 1521 to 1559. One might also ask why we should choose the Thirty Years' War as a random sample of history under multipolarity. Its selection surely skews the results. Why not choose as a starting point the year when seventeenth century economic contraction began and as a closing point the year when economic expansion resumed?

In short, I think that international scarcity/plenty must be defined far more rigorously if it is to form the foundation for theorizing about international politics. Moreover, its hypotheses must be logically so deduced that it may not predict both *a* and not-*a* with equal cogency. Finally, the theory's empirical fit is highly questionable: the first half of the sixteenth century was fraught with great-power wars, though an era of abundance by Midlarsky's measurements; the Thirty Years' War can hardly be considered a valid sample of seventeenth-century life under multipolarity; and the 15 years leading up to World War I were marked by an era of vigorous economic growth.

CAPTURING MIDLARSKY WITH THE OFFENSE–DEFENSE BALANCE

Midlarsky's alternative needs much more refinement before it can be productively applied to international politics. In the meantime, I believe that one of the elements of the offense–defense balance—the cumulative nature of material resources—does a very nice job in encompassing the theoretical territory Midlarsky would like settled. I hypothesized in my article that when the resources that constitute the sinews of power in an era are *available* and *extractable*, there would be increased instability. For example, I noted that in the early sixteenth century, grain was an available easily extractable power resource. It was available, being grown in convenient places, such as southern Italy; and it was extractable because the peasants who grew it had only the weakest preferences between exploitative feudal lords (pp. 477–78, 483, 485). This argument, of course, is precisely the opposite of Midlarsky's. My logic is quite simple: states will go to war, ceteris paribus, when there are rewards available and they can reap those rewards at low cost.

But there is more to the offense–defense balance than just resource issues. There are the military balance and strategic beliefs. Midlarsky interprets the evidence from sixteenth century Europe (in my view, mistakenly) only by ignoring these elements. He argues that sustained plenty erased the effects of a shift from multi- to bipolarity in the period. This could only be true if the level of stability across the two periods had been stable at a *low* level of violence. But in fact, it was 65 years of virtually continual warfare among the great powers, there being all of five years of peace during that period (see Table 4 of the article). Midlarsky's position would be sound if he could show that after this period of abundance, the level of instability increased *relative* to the previous period of plenty. Instead, the constantly high level of instability across the two periods of different polarity is consistent with a military balance of tactical offensive advantage, offensive strategic beliefs, and available and extractable power resources.

A comprehensive offense–defense balance is necessary, as well, to explain the Cold War. Midlarsky argues that this was a period of plenty but then acknowledges the relevance of nuclear weapons—implying that he knows that the military balance between offense and defense is a critical component of any theory of international politics. The high level of stability between the great powers was further reinforced by the low amounts of available and extractable resources. But we also know that while there was no war among the great powers, there was a fair amount of war fought by the great powers (either directly or through proxies) in the Third World. Soviet and U.S. involvement in the periphery is not consistent with an "era of plenty." It is consistent, however, with offensive strategic beliefs about credibility, bandwagoning, and dominoes.

In short, I suggest that the offense–defense balance can both capture the economic and strategic dynamics implied in Midlarsky's alternative and, through its two other components, account for outcomes left unexplained by "international desiderata."

CONCLUSION

I would like to present what I believe to be a fruitful combination of Midlarsky's alternative and the offense–defense balance and then suggest a historical case that could be a productive test for the new theory.

Midlarsky argues that polarity is a variable whose causal power is contingent upon the level of scarcity or plenty in the international system. In place of this formulation, I would argue that polarity has no causal power. Furthermore, it is international abundance/scarcity and the cumulative nature of material resources that are contingent. And their causal relevance depends on the other two components of the offense–defense balance: the technical military balance and strategic beliefs. The logic of this theory is that when a state is faced with declining economic growth (according to Midlarsky) or available and extractable power resources (according to me), it has an incentive to go to war to reap these benefits. (It is still an open empirical question as to whose logic—Midlarsky's or my own—is more robust in explaining when states expand. Midlarsky's theory assumes that states are driven by *need*; the cumulativeness element of the offense–defense balance assumes states are driven by *opportunity*.) But whether a state responds to these opportunities is *contingent* on whether offense has the advantage militarily and whether the decision maker's strategic beliefs about how the world works make expansionism look necessary.

I think it would be very fruitful to test this reformulated model against Midlarsky's in the case of the U.S.–Soviet competition during the Cold War. As I have argued, I think the offense–defense balance performs better because it not only predicts *no war* between the two superpowers (as would Waltz and Midlarsky also) but also accurately predicts the behavior of the United States and Soviet Union in the Third World—which neither Waltz nor Midlarsky were able to capture. But I do not think this at all disposes of the matter. With the increasing availability of Soviet archives in Moscow and similar access in Washington, a researcher can establish with a reasonable level of confidence what caused the peace, at least in the minds of the decision makers from 1945–60. Was it fear of nuclear war, as the offense–defense balance proponent would argue? Was it the confidence in one's security that comes with being a duopolist, as Waltz would argue? Or was it satisfaction with one's economic security in an expanding world economy, as Midlarsky would argue? Finally, was superpower competition in the Third World in fact due to high-level beliefs about the need to maintain credibility and to prevent bandwagoning and falling dominoes, as the offense–defense balance suggests?

In short, I believe that Midlarsky's alternative theoretical framework is flawed by underspecification but that its central thrust can be captured by the offense–defense balance. And I strongly agree that neither one of our alternatives will achieve any level of general validity until both are subjected to rigorous empirical testing in a large number of cases.

TED HOPF

University of Michigan

Notes

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1. I am very grateful to Mike Desch for developing this line of argument for me.

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NOTES FROM THE MANAGING EDITOR

FORTHCOMING IN JUNE, 1993

The following articles, research notes and controversies have been tentatively scheduled for publication in the June 1993 issue:

Larry Bartels. "Messages Received: The Political Impact of Media Exposure."

Shelley Burtt. "The Politics of Virtue Today: A Critique and a Proposal."

Cathy J. Cohen and Michael Dawson. "Neighborhood Poverty and African American Politics."

Joanne Gowa and Edward Mansfield. "Allies, Adversaries and International Trade."

C. F. Larry Heimann. "Understanding the *Challenger* Disaster: Organizational Structure and the Design of Reliable Systems."

George Klosko. "Rawls's 'Political' Philosophy and American Democracy."

Susanne Lohmann. "A Signaling Model of Informative and Manipulative Political Action."

Rebecca Morton. "Incomplete Information and Ideological Explanations of Platform Divergence."

Karen L. Remmer. "The Political Economy of Elections: Latin America 1980-1990."

Anne L. Schneider and Helen Ingram. "The Social Construction of Target Populations: Implications for Politics and Policy."

Dennis F. Thompson. "Mediated Corruption: Some Implications of the Case of the 'Keating Five'"

Sidney Verba, Kay Lehman Schlozman, Henry Brady, and Norman H. Nie. "Citizen Activity: Who Participates? What Do They Say?"

Miriam Golden. "The Dynamics of Trade Unionism and National Economic Performance." A Research Note.

Rein Taagpera and Matthew Shugart. "Predicting the Number of Parties: A Quantitative Models of Duverger's Mechanical Effect." A Research Note.

BOOK REVIEWS

American Political Science Review

Book Review Contents

REVIEW ESSAY

THE CAUSES AND CONSEQUENCES OF DIVIDED GOVERNMENT: TOWARD A NEW THEORY OF AMERICAN POLITICS?
Cox, Gary W., and Samuel Kernell, eds. *The Politics of Divided Government*
  Fiorina, Morris. *Divided Government*
  Jacobson, Gary C. *The Electoral Origins of Divided Government: Competition in U.S. House Elections, 1946–1988*
  Mayhew, David R. *Divided We Govern: Party Control, Lawmaking, and Investigations, 1946–1990* . . . . David W. Brady 189

POLITICAL THEORY

Alford, C. Fred. *The Self in Social Theory: A Psychoanalytic Account of Its Construction in Plato, Hobbes, Locke, Rawls, and Rousseau* . . . . Mark E. Warren 195
Andelson, Robert V. *Commons without Tragedy: Protecting the Environment from Overpopulation—a New Approach*
  Grundmann, Reiner. *Marxism and Ecology* . . . . Robert V. Bartlett 196
Bohman, James. *New Philosophy of Social Science: Problems of Indeterminacy* . . . . Brian Fay 197
Cohen, Jean L., and Andrew Arato. *Civil Society and Political Theory* . . . . James Bohman 198
Elster, Jon. *Local Justice: How Institutions Allocate Scarce Goods and Necessary Burdens* . . . . John S. Dryzek 199
Fontana, Biancamaria. *Benjamin Constant and the Post-Revolutionary Mind*
  Kelly, George Armstrong. *The Humane Comedy: Constant, Tocqueville, and French Liberalism*
  Klaits, Joseph, and Michael H. Haltzel, eds. *Liberty/Liberté: The American and French Experiences* . . . . Graeme Garrard 200
Golding, Sue. *Gramsci's Democratic Theory: Contributions to a Post-liberal Democracy* . . . . Michael Hanchard 201
Inness, Julie C. *Privacy, Intimacy, and Isolation* . . . . Richard F. Hixson 202
Klosko, George. *The Principle of Fairness and Political Obligation* . . . . Donald Becker 202

Ring, Jennifer. *Modern Political Theory and Contemporary Feminism: A Dialectical Analysis* . . . . Somer Brodribb 204
Taylor-Gooby, Peter. *Social Change, Social Welfare, and Social Science* . . . . K. Fierlbeck 204

AMERICAN POLITICS

Alwin, Duane F., Ronald Cohen, and Theodore M. Newcomb. *Political Attitudes over the Life Span: The Bennington Women after fifty Years* . . . . Philip E. Tetlock 206
Black, Earl, and Merle Black. *The Vital South: How Presidents Are Elected*
  Hrebenar, Ronald J., and Clive S. Thomas, eds. *Interest Group Politics in the Southern States* . . . . Charles S. Bullock III 206
Campbell, John L., Jr., Rogers Hollingsworth, and Leon N. Lindberg, eds. *Governance of the American Economy* . . . . Alan Stone 208
Corrado, Anthony. *Creative Campaigning: PACs and the Presidential Selection Process*
  Sorauf, Frank J. *Inside Campaign Finance: Myths and Realities* . . . Theodore J. Eismeier 209
Durant, Robert F. *The Administrative Presidency Revisited: Public Lands, the BLM, and the Reagan Revolution*
  Stoker, Robert P. *Reluctant Partners: Implementing Federal Policy* . . . . Paul J. Quirk 210
Epstein, Barbara. *Political Protest and Cultural Revolution: Nonviolent Direct Action in the 1970s and 1980s* . . . . Paul Burstein 211
Evans, C. Lawrence. *Leadership in Committee: A Comparative Analysis of Leadership Behavior in the U.S. Senate*
  Fenno, Richard F., Jr. *The Emergence of a Senate Leader: Pete Domenici and the Reagan Budget* . . . . Christopher J. Deering 212
Fuchs, Ester R. *Mayors and Money: Fiscal Policy in New York and Chicago* . . . . Steven P. Erie 213
Gierzynski, Anthony. *Legislative Party Campaign Committees and the American State* . . . . Clyde Wilcox 214
Gilmour, John B. *Reconcilable Differences? Congress, the Budget Process, and the Deficit* . . . . John J. Coleman 215
Hess, Stephen. *Live from Capitol Hill: Studies of Congress and the Media* . . . . Charles M. Tidmarch 215
Jencks, Christopher. *Rethinking Social Policy: Race, Poverty, and the Underclass*
  Jencks, Christopher, and Paul E. Peterson, eds. *The Urban Underclass*
  Mead, Lawrence M. *The New Politics of Poverty: The Nonworking Poor in America* . . . . Jennifer L. Hochschild 216
Lindsay, James M. *Congress and Nuclear Weapons* . . . . Jeffrey W. Knopf 218
Mizruchi, Mark S. *The Structure of Corporate Political Action: Interfirm Relations and Their Consequences* . . . . Michael C. Munger 219
Nelson, Albert J. *Emerging Influentials in State Legislatures: Women, Blacks, and Hispanics* . . . . John F. Bibby 220
Parenti, Michael. *Make-Believe Media: The Politics of Entertainment* . . . . Doris A. Graber 221
Petracca, Mark P. *The Politics of Interests: Interest Groups Transformed* . . . . Burdett Loomis 221
Samar, Vincent J. *The Right to Privacy: Gays, Lesbians, and the Constitution*
  Wenz, Peter S. *Abortion Rights as Religious Freedom* . . . . Raymond Tatlovich 222
Tanur, Judith M., ed. *Questions about Questions: Inquiries into the Cognitive Bases of Surveys* . . . . Santa Traugott 223
Thomas, Robert D., and Richard W. Murray. *Progrowth Politics: Change and Governance in Houston* . . . . Dennis R. Judd 224
Vogel, Ronald K. *Urban Political Economy: Broward County, Florida* . . . . Thomas R. Dye 225
Weiher, Gregory R. *The Fractured Metropolis: Political Fragmentation and Metropolitan Segregation* . . . . Susan E. Clarke 225
Weir, Margaret. *Politics and Jobs: The Boundaries of Employment Policy in the United States* . . . . Donald C. Baumer 226
Wilcox, Clyde. *God's Warriors: The Christian Right in Twentieth-Century America* . . . . James L. Guth 227
Wilson, James Q. *On Character* . . . . Aaron Wildavsky 228

COMPARATIVE POLITICS

Anderson, Jeffrey J. *The Territorial Imperative: Pluralism, Corporatism, and Economic Crisis* .... Christopher S. Allen 230

Bashevkin, Sylvia B. *True Patriot Love: The Politics of Canadian Nationalism*
Cairns, Alan C. *Charter versus Federalism: The Dilemmas of Constitutional Reform*
Weaver, R. Kent, ed. *The Collapse of Canada?* .... Barry Cooper 231

Blondel, Jean, and Jean-Louis Thiebault, eds. *The Profession of Government Minister in Western Europe*
Campbell, Colin, S.J., and Margaret Jane Wyszomirski, eds. *Executive Leadership in Anglo-American Systems* .... Barbara Kellerman 232

Cohen, Robin, and Harry Goulbourne, eds. *Democracy and Socialism in Africa* .... Ronald H. Chilcote 233

Dietz, Henry, Jerrold Elkin, and Maurice Roumani, eds. *Ethnicity, Integration, and the Military*
Zagorski, Paul. *Democracy vs. National Security: Civil–Military Relations in Latin America* .... David Pion-Berlin 234

Feldman, Allen. *Formations of Violence: The Narrative of the Body and Political Terror in Northern Ireland* .... David E. Schmitt 235

Finn, John E. *Constitutions in Crisis: Political Violence and the Rule of Law* .... David C. Rapoport 236

Friedman, Edward, Paul G. Pickowicz, and Mark Selden. *Chinese Village, Socialist State*
Goldstein, Avery. *From Bandwagon to Balance-of-Power Politics: Structural Constraints and Politics in China, 1949–1978* .... Richard Baum 237

Glaser, William A. *Health Insurance in Practice: International Variations in Financing, Benefits, and Problems* .... Theodore R. Marmor 239

Goodman, John B. *Monetary Sovereignty: The Politics of Central Banking in Western Europe* .... Susanne Lohmann 240

Kazemi, Farhad, and John Waterbury, eds. *Peasants and Politics in the Modern Middle East*
Kriger, Norma J. *Zimbabwe's Guerilla war: Peasant Voices*
Tareke, Gebru. *Ethiopia: Power and Protest, Peasant Revolts in the Twentieth Century* .... Leonardo A. Villalón 242

Kellas, James G. *The Politics of Nationalism and Ethnicity* .... Charles F. Furtado, Jr. 244

Kull, Steven. *Burying Lenin: The Revolution in Soviet Ideology and Foreign Policy*
Urban, Joan Barth, ed. *Moscow and the Global Left in the Gorbachev Era* .... Kimberly Marten Zisk 245

Mayer, Ann Elizabeth. *Islam and Human Rights: Tradition and Politics* .... Jill Crystal 246

Moon, Bruce E. *The Political Economy of Basic Human Needs* .... Fred C. Pampel 247

Philip, George. *The Presidency in Mexican Politics* .... Judith Gentleman 247

Rau, Zbigniew, ed. *The Reemergence of Civil Society in Eastern Europe and the Soviet Union* .... John Bendix 248

Rosenbaum, Arthur Lews, ed. *State and Society in China: The Consequences of Reform*
Wasserstrom, Jeffery N., and Elizabeth J. Perry, eds. *Popular Protest and Political Culture in Modern China: Learning from 1989* .... Paul G. Pickowicz 249

Scully, Timothy R. *Rethinking the Center: Party Politics in Nineteenth and Twentieth Century Chile* .... Ronald H. McDonald 250

Silver, Harold, and Pamela Silver. *An Educational War on Poverty: American and British Policy-making, 1960–1980* .... John F. Witte 251

Willerton, John P. *Patronage and Politics in the USSR* .... Thomas F. Remington 252

INTERNATIONAL RELATIONS

Brzoska, Michael, and Peter Lock, eds. *Restructuring of Arms Production in Western Europe* .... Andrew Moravcsik 253

Daalder, Ivo H. *The Nature and Practice of Flexible Response: NATO Strategy and Theater Nuclear Forces since 1967* .... John S. Duffield 253

David, Steven. *Choosing Sides: Alignment and Realignment in the Third World*
Jackson, Robert H. *Quasi-States: Sovereignty, International Relations, and the Third World* .... S. N. MacFarlane 254

Efrat, Moshe, and Jacob Bercovitch, eds. *Superpowers and Client States in the Middle East: The Imbalance of Influence* .... Michael N. Barnett 256
Encarnation, Dennis. *Rivals Beyond Trade: America Versus Japan in Global Competition* .... Susan Strange 257
Holbraad, Carsten. *Danish Neutrality: A Study in the Foreign Policy of a Small State* .... Alan K. Henrikson 258
Job, Brian L., ed. *The Insecurity Dilemma: National Security of Third World States* .... Terrence Lyons 259
Karp, Regina Cowen, ed. *Security with Nuclear Weapons? Different Perspectives on National Security* .... Edward A. Kolodziej 259
Kennedy, Paul, ed. *Grand Strategies in War and Peace* .... Harvey Starr 261
Leffler, Melvyn P. *A Preponderance of Power: National Security, the Truman Administration, and the Cold War* .... Alan C. Lamborn 261
Pipes, Daniel, and Adam Garfinkle, eds. *Friendly Tyrants: An American Dilemma* .... Ole R. Holsti 262
Princen, Thomas. *Intermediaries in International Conflict* .... Linda P. Brady 263
Reisman, Michael, and James E. Baker. *Regulating Covert Action: Practices, Contexts, and Policies of Covert Coercion Abroad in International and American Law* .... Frances V. Harbour 264
Telhami, Shibley. *Power and Leadership in International Bargaining: The Path to the Camp David Accords*.................. Steven Greffenius 265

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THE CAUSES AND CONSEQUENCES OF DIVIDED GOVERNMENT: TOWARD A NEW THEORY OF AMERICAN POLITICS?

DAVID W. BRADY *Stanford University*

The Politics of Divided Government. Edited by Gary W. Cox and Samuel Kernell. Boulder: Westview Press, 1991. 270p. $55.00 cloth, $15.95 paper.

Divided Government. By Morris Fiorina. New York: MacMillan Publishing Company, 1992. 138p. $10.50 paper.

The Electoral Origins of Divided Government: Competition in U.S. House Elections, 1946–1988. By Gary C. Jacobson. Boulder: Westview Press, 1990. 152p. $44.50 cloth, $15.95 paper.

Divided We Govern: Party Control, Lawmaking, and Investigations, 1946–1990. By David R. Mayhew, New Haven: Yale University Press, 1991. 228p. $25.00.

It is somewhat ironic that these four books on divided government have come out just months before twelve years of divided government has, at least temporarily, ended. The post-World War II era of American politics has been characterized by divided government—twenty-six years of Republican Presidents and at least a Democratic House of Representatives and two years of a Democratic President and a Republican Congress compared to twenty years of unified government with all but two of these years being unified Democratic government. For such a long-lived phenomenon, divided government generated little serious attention until James Sundquist's (1988) *Political Science Quarterly* article called for a new theory of divided government.[1] Sundquist's article and the election of George Bush to the presidency generated a spate of work on divided government. Bush's win was crucial in that previous Republican presidential victories had been viewed as aberrations—Eisenhower, a war hero and the Korean War in 1952, Nixon and an America divided over Vietnam, and Reagan running against the backdrop of Iran and a poor economy. No special circumstances surrounded George Bush's election in 1988 nor was it an incumbent reelection. Thus, political scientists and reporters alike came to believe that Republican Presidents and Democratic Congresses were here to stay.

These four books and numerous other books and articles on divided government followed the 1988 reelection results. Many of these works bemoaned the effects of divided government, calling the effects policy gridlock. Sundquist (1988), among others, claimed that divided government was both inefficient and irresponsible. Reformers sought ways to change the system to lessen the probability of divided government. Political scientists on average were concerned less with the normative argument; instead, they concentrated on the causes of divided government. These four books deal with both the causes and consequences of divided government. This essay begins by first examining the various authors' views of the causes of divided government and then explaining the authors' views of the consequences of divided government. Before turning to this task, it is worth noting how important the study of divided government is to the field of American politics.

Fiorina correctly points out (pp. 3–5) that the realignment synthesis of American politics has largely played out as a vehicle for understanding how elections, institutions, and policy have fit together over time. The study of divided government has the potential to replace the realignment synthesis as the major paradigm. Voters' preferences expressed through elections, institutional structures, and public policy outputs are all presumably different in an era of divided government. Political parties, the dominant vehicle for change under the realignment synthesis, are structurally different. Moreover, politicians behave differently when executives are of one party and legislatures of the other. Given the potential of divided government studies to redefine a synthesis, the most important question is, does divided government make a difference? This question is often put in other ways, such as the often heard claim that unified gridlock occurred under President Carter. Throughout this essay and in the conclusion, I try to judge how far these authors take us toward a theory that explicates the causes of divided government and makes predictions about the direction of public policy under both divided and unified government.

CAUSES OF DIVIDED GOVERNMENT

There are two distinct approaches to the study of the causes of divided government at the national level. The first focuses on the Democratic Congress as the source of divided government while the second seeks to explain why Republicans have dominated recent presidential elections. Gary Jacobson's book *The Electoral Origins of Divided Government* focuses on the Democratic Congress (essentially, the House of Representatives) as the origin of divided government. His argument is that the standard interpretation of why the House is Democratic—incumbency advantage—is seriously flawed. That is, the argument that Democrats control the House because incumbents win reelection, and there are more Democratic incumbents is, according to Jacobson, not the major reason why Democrats win House elections. Jacobson reviews the literature on incumbency and finds it inadequate largely because Democrats win open-seat elections more often than do Republicans, i.e., Democrats capture more former Republican seats that come open than Republicans capture former Democratic seats in open elections.

The question then becomes, why are Democrats so successful in House races? Jacobson argues that Democrats have better candidates (as measured by levels of previous political experience) and that they have better positions on issues. The previous political experience hypothesis is clearly and convincingly demonstrated. The argument that Democrats have better positions on issues is harder to demonstrate and hangs on a distinction between local and national issues. This distinction is crucial; John Petrocik, in his contribution to the Cox and Kernell volume, similarly claims that Republican presidential victories are a result of Republican "ownership" of crucial issues such as managing the economy. Both claims cannot be true unless we distinguish between electoral levels. In full form, the Petrocik argument is that at the district level voters want as representatives individuals who will fight for and deliver government goods and services beneficial to the district. Thus, for example, whoever represents eastern Montana will have to support agricultural policies that benefit the wheat farmers and ranchers who dominate the economy of eastern Montana. Beyond price supports and programs benefiting their interests, the voters in eastern Montana do not wish their tax burdens to increase so as to support programs aiding other interests. If voters in districts favor policies that benefit them, they will elect candidates who favor governmental action at the local level without commitments, one way or the other, favoring governmental actions benefiting other districts. Since Democrats are on average more favorable to governmental action, their policy positions will do "better."

How, if Democrats have "better" positions on local issues, can this be squared with claims that Republicans own issues on the national level? The answer lies in how local issues aggregate at the national level. If voters saw their House incumbent as spatially distinct from the position of the member's party, then the levels-of-election argument could be compatible with both Jacobson's and Petrocik's arguments. I analyzed National Election Study (NES) data for all presidential election years from 1960 through 1988 to determine if such a difference exists. Every year that the NES asked respondents to place themselves on a liberal (1) to a conservative (7) scale, the median voter was at 4, the Democratic party at about 2, and Republicans at 6. Their Republican representatives, however, averaged 4.5, while their Democratic representatives averaged 3.6. In short, members can support government programs beneficial to their districts and not get tarred with the too-liberal or too-conservative brush borne by their national parties. When all of these locally popular governmental programs are aggregated nationally, the result is that the majority Democratic party in the House sounds like a cacophony of special interests saleable one at a time locally but not coherently aggregated. From this perspective, which party is the majority in the House does not matter; for majority parties to maintain their status, the individual members have only to support government programs which benefit their constituents.

Republicans, a la Petrocik, could then own certain national issues like the economy because citizens think of the country as a whole when voting for President. Since Republicans are in the minority in the House, they have a more homogeneous constituency, and their presidential candidates can take positions that are more appealing to a national constituency. This argument is compatible with, even underlies, Martin Wattenberg's essay (also in Cox and Kernell) on how the party with the quickest, cleanest primary victory wins the presidency. His thesis is that the institution of the primaries generates left-of-center Democratic primary voters and right-of-center Republican primary voters, which in turn pulls candidates away from the centrist November voters. The party with the longest and most decisive primary ends up with a candidate too far from the center and with too little time to move there. A Democratic majority in the House whose members support hundreds of localized governmental programs creates a heterogeneous party, which in general will have a hard time easily nominating a centrist candidate. In sum, both Jacobson's claim that Democratic House candidates have "better" issue positions and Petrocik's and Wattenberg's arguments for Republican presidential ascendancy are compatible if we distinguish between electoral levels and assume that voters decide on different criteria, depending on the office being contested. Research up to this point has not clearly specified the linkage between congressional and presidential elections. Without such specifications, the crucial roles of issues in explaining divided government remains unknown.

Jacobson's second claim that Democrats have better candidates for House seats as measured by previous political experience is surely empirically true but remains a puzzle in that we do not know why Democrats have greater experience. Here Fiorina in *Divided Government* makes a major contribution. Fiorina documents the rise of divided government at the state level in American politics and shows that the major factor in the rise of divided government in the states is the decline of Republican control of the state legislatures in the post-World War II period. Fiorina offers some evidence that the decline of Republican control of state legislatures is concomitant with the rise of professionalized state legislatures. That is, the greater the likelihood that a state legislature meets full-time, has professional staff, and high pay for legislatures, the greater the likelihood that the legislature will be dominated by Democrats. Thus, an unintended consequence of the movement to professionalize state legislatures was to diminish the state base of potential Republican congressional candidates. Democratic dominance of state legislatures means that Democrats have a larger base of potential congressional candidates schooled in the art of hiring campaign teams, raising money, and running for election at the state level.

Underlying Fiorina's analysis is the implicit assumption that the opportunity costs for Republicans running for office and winning or losing are higher than for Democrats. One aspect of these opportunity costs is that potential Republican candidates know that, even if they win elections to the U.S. House, they will not be chairs of committees or subcommittees given the Democrats' forty-year dominance of the House. At the state level, a brief analysis of the data shows that Republicans in 1990 controlled over two-thirds of the ten legislatures that meet for less than six months every two years. Again, the idea is that Republican businesspeople, farmers, ranchers, and lawyers in a state like Wyoming can serve in the state legislature without giving up their careers. Suffice it to say that the question of opportunity costs across parties will and should generate a good deal of interesting research over the next decade.

Some scholars have tried to use the periods of divided government at the end of the nineteenth century as a vehicle to get a handle on the post-World War II period (see Stewart's conclusion in Cox and Kernell for a counterinterpretation). This research will not, in my opinion, prove very useful because the periods of divided government in the late nineteenth century differ in important ways. The major difference is that in the late nineteenth century, divided government occurred in off-year elections when the electorate changed the majority party in Congress. In the subsequent presidential election, the off-year House elections correctly predicted which party's presidential candidate would get the most voters. That is to say that in the late nineteenth century the presidential and congressional vote was highly correlated over pairs of elections. In the post World War II period and especially post-1964, the divided government phenomenon is driven by the rise of split-ticket voting and the level of the personal vote for incumbent members of Congress. The rise of the personal vote, especially in House elections, breaks the connection between congressional and presidential elections and scholarly attempts to determine the causes of and, ultimately, the consequences of divided government depend on understanding the nature of the presidential-congressional severance. Neither the electoral-levels theory discussed above nor the balancing theory discussed below can be used to explain divided government in the late nineteenth century.

The causes of divided government arguments in the Jacobson, Fiorina, and Cox and Kernell books feature two interpretations: one focusing on why the Democrats control the House; the other on why the Republicans control the presidency. I have argued that these two strands of research are compatible if one assumes that voters choose candidates at the House level on local issues and that voters look for different characteristics when making their choice on the presidential level. Another major interpretation accounting for divided government is the balancing theory.

Fiorina reviews this literature and offers several interpretations. Loosely, the idea is that at the national level voters choose Republican presidential candidates from the right to balance a leftist Democratic Congress. The underlying model is a spatial voter model where voters understand institutions and vote accordingly. In a pure median voter model, voters choose candidates closest to their positions whereas, in a balancing model, voters may choose to vote for a Republican presidential candidate who is actually farther from their own position than is the Democratic presidential candidate. Voters do this because they realize (or act as if they realize) the policy consequences of having both a Congress and a President to the left of their ideal positions. Therefore they vote for a Republican presidential candidate to their right to cancel out the Congress and bring policy closer to their ideal point. Fiorina points out that the number of voters choosing this way need not be large in order for balancing voters to determine outcomes at the margin.

The balancing theory is appealing because it is theoretically straightforward, takes institutions into account, and squares fairly well with recent results at the national level. As Fiorina points out, however, there are a number of states with Republican legislatures and Democratic governors where balancing theories would have to be seriously modified to account for these results. In addition, balancing models have not tested any alternative explanations such as different issues at different levels. Can a balancing theory account for President-elect Bill Clinton's victory in 1992? Until either theory is more fully specified or is tested against alternatives, we should view the theory as one of a set of plausible but unproven theories of the causes of divided government.

CONSEQUENCES OF DIVIDED GOVERNMENT

Not knowing the causes of divided government constitutes a major problem for determining its consequences. If, for example, the origins of divided government lie in Democratic control of the U.S. House, the Democrats would be a heterogeneous, cacophonous national party generally unable to put together a winning presidential campaign. Then, when Democratic Presidents are elected, they may be incapable of governing. For example, the Carter presidency may well have been what we could have predicted given the nature of his electoral coalition, which means also the standard interpretation that his presidency was flawed because he had too many Georgians or that his legislative liaison was weak would be incorrect. Indeed, it may well be the case that Democrats can only put together winning coalitions when there is a sitting Republican President and a slow economy. The only victorious Democrats in the post-primary period (1964) have been Jimmy Carter and Bill Clinton, both of whom ran after at least eight years of Republican presidencies and campaigned on a theme of change. The point is that a central question about the consequences of divided government is, does gridlock arise equally under both divided and unified government?

The answer to this question depends upon the definition of gridlock. Beginning with Sundquist, many scholars have clearly come down on the side of gridlock under divided government. For a long time, this view predominated, and most political scientists, in my opinion, still believe that the consequences of divided government are stalemate and gridlock. David Mayhew, in *Divided We Govern,* challenges this view. Mayhew does not attempt to determine the cause of divided government; rather he essentially takes it as exogenous and examines the consequences in terms of policy outputs (i.e., major policy changes) and process (i.e., the frequency of congressional investigations). Answering this question is a formidable task because one has to make tenuous assumptions about what major policy change is and what it means to say that the system is or is not responding to the public's policy preferences. Mayhew does an admirable job with this difficult task. His assumptions about major policies and his classification of legislative acts are well thought out and carefully derived. His conclusion is that divided government has not produced gridlock in either policy or process. In general, the Congress and the President have responded to changes in the public mood by passing legislation roughly in accord with the public's needs and desires. This is strong counterevidence to those who believe that divided government yields gridlock.

The essays by Kernell, McCubbins, Cox and McCubbins, Stewart, and Cox and Kernell in *The Politics of Divided Government* come to quite a different conclusion about consequences than does Mayhew. Cox and Kernell argue that partisan differences magnify the institutional separation of powers. This magnification leads to intense institutional conflict wherein policy decisions are the result of an institutionally structured bargaining process where each party has a veto. In this bargaining game, three strategies are available: (1) either the President or Congress goes it alone without the other branch, (2) there is an appeal to public opinion, or (3) both branches bargain within the Beltway. Depending on factors such as the location of the status quo and the reversion point, public policy outputs are affected by divided government. McCubbins' study of federal spending and deficits argues that divided control of Congress leads to increased deficits, while Cox and McCubbins argue that divided government can produce larger changes in policy than would alteration of parties in power. Stewart finds that in the late nineteenth century divided control of government affected government spending levels. In short, these authors' findings differ from Mayhew's. Most of these studies are grounded in theories which specify the roles of actors, institutions and parties in the formation of public policy. Yet it is unclear what they would predict about policy in the next four years. Mayhew, who does not articulate a theory of divided government, implicitly makes a clear prediction about the next four years based on his observations: that they will not be much unlike the last four. Still, the very different conclusions need to be reconciled.

One crucial question regarding the consequences of divided government is, by what mechanisms does divided government work to shape final policy outputs? Here, articles by Matthew McCubbins and Samuel Kernell on the role of institutional vetoes are helpful. Republican presidents facing Democratic congresses use threats of vetoes or actual vetoes to drive policy toward their preferred positions. The difficulty with going beyond this statement is that we have no studies showing where the policy would have been without the threatened or actual veto. For example, how much different would the 1977 Energy Act have been with a Republican President in office? Under a unified Democratic government, President Carter proposed an energy bill that emphasized conservation over exploration. In the House, using a special feature of the 1974 reform, an ad hoc committee passed the Carter bill with few changes. In the Senate, however, Senator Russell Long (D–LA) used his committee to change dramatically Carter's legislation. The resultant hundred-plus conference committee passed a final bill that did not vary much from what a reelected Gerald Ford would likely have passed through Congress.

A secondary issue regarding vetoes is their effects on public confidence and on the campaign efforts of Presidents who use them. President Bush, for example, used the veto to push the 1991 Civil Rights Act closer to his position. In the end, however, Bush could not take credit for the Act given his continual threats to veto. The Congress could not take full credit for the Act since the President had called it a quota bill. In contrast, bipartisan credit claiming surrounded the 1964 Civil Rights Act and the 1965 Voting Rights Act. McCubbins' and Kernell's articles correctly focus attention on the importance of the veto and, I hope, will generate further research in this area.

Students of the consequences of divided government differ in the extent to which they believe that stalemate or gridlock results. Mayhew's study is the best and most comprehensive study of the overall effect of divided government. Those arguing the case for stalemate have no comprehensive study, yet their view has dominated public perceptions to the extent that two-thirds of Americans now believe that divided government is not a good thing. In the following section, I shall argue that we should attempt to put together the two halves—causes and consequences—and generate predictions from such comprehensive theories.

CONCLUSIONS

The first unified government since 1980 should provide an excellent opportunity for testing theories of divided government. The first and most obvious question is about the policy consequences of unified government under President Clinton with the possibilities ranging from gridlock through no consequences to significant government action. Answers to this question should depend on the causes of divided government. None of the various theories offered in these books explicitly make such predictions. In fairness to the authors, they are often interested in a more limited question (e.g., why is the House Democratic) or in explaining the past or specific policies. Nevertheless, any theory purporting to explain divided government should tie together causes and consequences such that predictions about the Clinton presidency may be made.

Those who believe that divided government causes gridlock should be willing to say that uniform government results in significant government activity. Why have so few been willing to say at least this much? My suspicion is that the Carter presidency from 1976 to 1980 (which can hardly be characterized as a period of significant change) makes predictions difficult in lieu of a connecting theory. Jacobson is essentially silent on the question of consequences; Fiorina sees the complexity and suggests answers without ever fully placing a solid bet on any of them; and Mayhew implies that it would not make much of a difference since divided government has not led to gridlock. Balancing theories implicitly hold that divided government yields centrist or status quo policies. That is, the theory holds that the Democratic Congress is left of the median voter while Republican presidential candidates are right of the median voter. Since voters take this into account when choosing Republican Presidents, it follows that divided governments constrain (left) congressional policies as well as (right) presidential policies. Electing a Democratic President slightly left of center logically should move policy left, somewhere between the Democratic President and the Democratic Congress. Studies of specific policy areas might, depending upon the location of the status quo, reveal little movement in given areas, but overall government activism should increase and policy should shift leftward. That is, however, exactly the prediction this model should have made for the Carter Administration.

A theory of divided government based on interests located in congressional districts yields quite another prediction. Regardless of which party controls the presidency, such a theory would argue that interests dominate the policy process and that the median member of Congress within various policy areas will dominate policy results. On this view, a Clinton presidency will not differ much from a Bush or a Carter presidency since what drives policy is the distribution of interests over states and districts, the organization of the House and Senate committees, and the location of the median House and Senate member. Change in health care policy, for example, will be hard to come by given the array and placement of relevant interests in the Congress. Moreover, the final health care policy result of a Clinton presidency will be very similar to the Bush proposal—a mix of private and public care with controls placed on Medicare expenditures. Likewise, policy on Corporate Average Fuel Efficiency, which failed to pass in 1990 and 1991, will still have Democrats like John Dingell (Michigan) and Bennett Johnston (Louisiana) opposing higher standards.

An implicit difference between theories predicting (if they predicted) change with unified government and those predicting gridlock is how they view political parties. If parties are viewed as more than the aggregate of the preferences of their members' constituencies, then one would predict coherent government action. If parties are simply the sum of constituent preferences, then legislation driving policy in a coherent direction (left in the case of the Clinton presidency) will be difficult to find. According to my calculations, the median House member in the next Congress is in Representative Charles Stenholm's (D–TX) Conservative Democratic Caucus, and overall policy results will be determined by that group's position. Understanding divided government's causes and consequences at root demands a theory of political parties which ties together interests, elections, and member behavior in committees and on the floor, and an understanding of how these arrangements vary when the President is of the same party as the congressional majority.

Research about the role of congressional parties in the policy process converges on two widely differing interpretations: one that party matters and the other that party is simply the aggregate of member and, ultimately, constituent preferences. Under the strong party view, the President bargains with the majority party median; while under the non-partisan view, the President bargains with the floor median voter. Assuming a Clinton presidency (where his position lies between the Democratic party median and the congressional median), the strong party view suggests that policy will move left toward the party median; while the non-partisan view suggests that Clinton will deal with the median floor voter and policy will not move left.

All of these books increase our understanding of divided government. The Jacobson book raises the level of debate about congressional elections and will for some time shape the field. Mayhew's work on the consequences of divided government is a powerful, well-argued counter to the divided government gridlock scenario. No one examining the consequences of divided government will be able to escape its influence. Articles by Petrocik, Wattenberg, McCubbins, and Kernell (in Cox and Kernell) stand out as important contributions to the study of divided government. The Fiorina book is an excellent review of the literature and, a la V.O. Key Jr., contains numerous insightful hypotheses that should keep a generation of doctoral students busy working on divided government dissertations. In short, progress toward understanding divided government has been made, and, more importantly, new questions have been raised.

Having said this, it is nevertheless the case that a theory which combines the causes and consequences of divided government, yielding predictions about unified government, has yet to be devised. Some might argue that this standard is too high in that it asks for a general theory of government. My view, however, is that unless we, as a profession, can offer clear theories of how elections, institutions, and policy are connected and deduce predictions from these theories, we shall simply be telling ad hoc stories. Political scientists should look on the next four years of unified government as an opportunity to develop and test general theories of American politics.

Notes

1. James L. Sundquist. "The New Era of Coalition Government in the United States." (*Political Science Quarterly* 103 [1988]).

POLITICAL THEORY

The Self in Social Theory: A Psychoanalytic Account of Its Construction in Plato, Hobbes, Locke, Rawls, and Rousseau. By C. Fred Alford. New Haven: Yale University Press, 1991. 229p. $30.00.

Contemporary political theorists often approach the self through problems of metaphysical foundations, epistemology, community, or structures of discourse. The cost, Fred Alford argues in this interesting and often original book, is that problems intrinsic to the self are eclipsed. And this in turn obscures the possibilities of the mature and rational self that contemporary theorists often value. Alford's aim is to recover a concern with "real selves," by which he simply means that problems of psychology have their own irreducible autonomy. What makes political theorists such as Plato, Hobbes, Locke, Rousseau, and Rawls "great," in Alford's view, is that they understand psychological issues as distinct, however interrelated with other kinds of analysis. But in recovering these concerns, Alford also wishes to show that these great thinkers deal with dangers of the self through strategies that contain, split, and otherwise weaken the self, undermining the mature and autonomous selves they claim to seek. In other words, a dialectic of enlightenment is reflected in the canon, not just in its overly narrow conceptions of reason but also in its psychological strategies.

Alford measures these theorists against a model of the self that combines Heinz Kohut with Lacan (chap. 2). Kohut understands the self to develop through narcissistic "projective identifications" onto people, things, and ideas. To the extent that the self locates its ideals and ambitions within these exterior objects, they become "selfobjects," essential to self-development. Thus, the self grows not by rejecting infantile ("archaic") narcissism but by progressively distinguishing its "selfobjects." This process allows for social relationships, mutual enrichment, empathy, and social coordination. But Kohut errs, in Alford's view, by seeing the world as if it were only "selfobjects"; the world tends to collapse into a vast extension of the self. For this reason, Alford supplements Kohut's model with Lacan. Because Lacan understands the self as a "lack"—indeed, as a kind of narcissistic reaction formation against external and internal reality—he also emphasizes the irreducible *otherness* of the world. Indeed, for Lacan, in contrast to Kohut, reason itself may express a narcissistic rage against otherness, a project of mastery that fails to recognize otherness. While Alford sees a good deal more substance in the self than Lacan (e.g., the self can be a relatively stable "illusion" depending on the continuity of "selfobjects"), Lacan warns against theories that read the world as if it followed from the needs of the self. By combining Kohut and Lacan, Alford can examine those tensions within the self that can be mediated but not resolved, tensions that follow from the self's need to establish its own subjectivity through introjection and projection, combined with the fact that self-caused subjectivity is inherently limited. Maturity is the self's ability to craft the self through the world while recognizing that the world is not a reflection of its will. What is problematic about the self from a political perspective is not, according to this model, innate aggressiveness or destructiveness but the narcissistic desire, so evident in very young children, to bend the world to one's will.

Alford measures the great thinkers against this model of the self, viewing their ideals as means of crafting the self and containing narcissistic desires. Surprising but often convincing is the argument that these great thinkers recommend ideals destructive of mature selves. Alford does not have to provide idiosyncratic readings to make this case but merely focuses our attention on problems of the self within the texts, problems we tend not to notice because we are often attending to questions of ethics, epistemology, models of political judgment, views of power, and the like. Plato's idealism, for example (chap. 3–4), succeeds in containing desire but only by recommending that the self assimilate itself to ideals that are, psychologically speaking, narcissistic fantasies of perfection. Such ideals exclude attachments to anything imperfect, that is, to the world of people and things. When Platonic ideals are introjected into the self (as an ironic reading of the *Republic* recommends), desire is simultaneously idealized and repressed. Assuming that such a self could be achieved, it would be a less dangerous self but also unengaged, undeveloped, and immature.

Alford's accounts of the self in other thinkers are equally devastating. Hobbes (chap. 5) views the self as a purely archaic entity and describes a sovereign who is an idealized projection of total power—again, a narcissistic fantasy. Could such a political society be created, it would check narcissistic desires but only by preventing the self from maturing. Locke (chap. 6) splits the self, channeling narcissistic desires into property acquisition. In an ingenious interpretation of Rawls (chap. 7), Alford shows that behind the seemingly abstract metaphysics of the original position is a concern with self-esteem and narcissistic injury to the self. The difficulty is that Rawls implicitly holds that the original position protects against narcissistic injury; and this allows him to avoid dealing with conditions of a mature, rational self. Rousseau (chap. 8), unlike other thinkers, resolves the conflict between self and other by dissolving the self and denying difference. Alford concludes (chap. 9) that the combination of autonomy and civilized society eludes these thinkers. The rational self remains an illusive ideal.

While I find Alford's argument original, provocative, and well taken, his model of the self is missing a dimension that his ideal of a mature, autonomous self requires. Following Kohut, Alford tends to conceive social bonding, coordination, and recognition in terms of affective projections and transferences while understanding maturity as a Lacanian recognition of otherness. Although this approach presupposes a cognitive dimension to the self (since this is the locus of recognition of otherness), the powers of cognitive capacities have no real place in the model. For example, the model fails to connect cognitive capacities and linguistic interaction—an important omission, since mature politics ought to rely on the binding force of discourse. And it means that the conflicts between society and self, while they should never be underestimated, may appear more intractable than they really are.

Georgetown University MARK E. WARREN

Commons without Tragedy: Protecting the Environment from Overpopulation—a New Approach. Edited by Robert V. Andelson. Savage: Barnes & Noble, 1991. 198p. $34.50.

Marxism and Ecology. By Reiner Grundmann. New York: Oxford University Press, 1991. 324p. $67.00.

With one or two notable exceptions, political scientists in the 1960s and early 1970s were slow to recognize and address environmentalism and the environmental problematique. When substantial research on environmental politics finally did begin to appear, most done through the mid-1980s entailed relatively easy and unimaginative applications of the tried-and-true theories, approaches, and methods of mainstream political science. Fortunately, however, the past half-dozen years have seen the publication of a significant number of more conceptually innovative works, works that build upon theoretical insights deriving from the unique political questions raised by the nature of the environmental problematique. The whole field has grown quantitatively at the same time. Increased international attention to environmental issues has inspired an even greater number of attempts to explain, illuminate, and even answer the questions raised by the environmental movement. Consequently, original pioneering research is still dwarfed by a much larger number of more pedestrian publications consisting mostly of works that attempt application of familiar theories and analytic methods to environmental problems or issues. Both *Commons without Tragedy*, edited by Robert V. Andelson, and *Marxism and Ecology* by Reiner Grundmann fall into this latter category.

Commons without Tragedy is a curious book. Its subtitle suggests a focus on overpopulation, yet half of the contributed chapters address overpopulation only secondarily. The subtitle also suggests "a new approach"; but we learn in the preface that the central purpose of the book is to reassess an old approach, the theories of Henry George. The aim, we are told, is to explore and promote the ideas of this nineteenth-century philosopher and reformer, notably his single tax on the rent of land exclusive of improvements, as relevant for avoiding and resolving potential commons tragedies. Expectations thus adjusted, we plow ahead to find, after the first two chapters by Roy Douglas and Robert V. Andelson, that George is rarely mentioned.

Nowhere in the book is there a critical exegesis of George's writing as relevant to the commons problem, nor are there more than a handful of passages quoted. Surely there is a contemporary critical literature on "the premier social philosopher to have been born in the United States" (p. ix)? None of the contributors to this book acknowledges the absence of any such literature; neither do they recognize existing literature on George as useful, worthy of comment, or requiring response or accommodation.

The excuse for remaining at a level so general and abstract (as well as vague, uncritical, and speculative) is that "the present work is not designed to develop to the full the question of how the 'commons' should be managed, but to lay down certain guiding principles which may help towards such a solution" (p. 25). Conveniently, political realities and the inertia of existing institutions, cultures, and legitimizing justifications are ignored, and practical matters such as the difficulties of calculating and administering a land rent tax are given short shrift. This excuse for loose argumentation, seasoned with the zeal of believing reformers, leads some of the authors to advance claims for a simple solution to a set of complex problems (of which overpopulation is only one) that are imbedded in an interplay of numerous social and natural processes.

The quality of the individual chapters varies widely. Contributions to the theoretical literature on the politics and policy of commons management are made in the chapters by Robert V. Andelson and Garrett Hardin. Andelson, in the chapter that inspired compilation of the book, concludes that a Georgist system would satisfy all of Hardin's criteria for preventing commons tragedies, securing "the advantages of both commons and enclosures with none of the disadvantages of either" (p. 41). He does not oversimplify the ecological issue but argues that a Georgist remedy would provide ecological side benefits though still leave much to be done. Hardin's concluding chapter is, as usual, eloquent and provocative. It is of interest if for no other reason than that it contains his latest reflections on his famous commons thesis.

A chapter by Fred Harrison is also of value for his incisive review of population theory deriving from Malthus and his analysis of the links between land tenure systems and demographic growth. The remaining chapters are likely to be of little interest to political scientists.

There are few specific points of comparison between this book and *Marxism and Ecology*, by Reiner Grundmann, aside from the obvious focus on another even more preeminent philosopher and reformer of the nineteenth century. The most surprising point of contrast must certainly be Grundmann's conclusion that "private property cannot count as a prime cause of ecological problems" (p. 46).

Grundmann's book is a revised Ph.D. thesis published in the Oxford University Press Marxist Introductions series. The offered analysis of Marx is critical and as specific and concrete as neo-Marxist analysis ever is. Unfortunately, the language used to convey the argument is occasionally more impenetrable than might be excused by the abstruseness of the concepts and literatures being used. Not surprisingly, Marx and his writings are addressed on virtually every page. An extensive secondary literature is addressed and cited. It is, significantly, a book on "Marxism and ecology" rather than "ecology and Marxism." From the beginning, there is no doubt which discourse is prior and has priority for Grundmann.

Marxism and Ecology is not a reconstruction of a green Marx: "For we all know Marx was no ecologist, even if he could have been one" (p. 6). Grundmann begins with the conclusion that Marx's theory of value offers no illumination for ecological problems. Grundmann turns away from Marx's political economy and toward the philosophical Marx, "giving preference to an approach which locates Marx in a philosophical tradition and connects his thoughts to contemporary social theory and interpretations of his work" (p. 8). Grundmann concedes that two of Marx's expectations have been disproved, namely, "that science and technology would create an intelligible and controllable world" and "that only capitalist relations stand in the way of such a goal" (p. 8). Rather, Grundmann argues that some productive forces are not inherently harmful to the environment and that property relationships are but one among many factors causing ecological problems.

Thus, Grundmann is interested in Marx's contribution to our understanding of concepts of nature, human nature, technology, technological development, division of labor, social evolution, and emancipation. Grundmann goes well beyond a purely Marxist analysis, offering a systems-theoretical reformulation of historical materialism and concluding with a discussion of communism, markets, and central planning.

Anchored as it is in a Marxist worldview, epistemology, and analytic methodology, *Marxism and Ecology* is certain to be of far greater value and interest to general Marxist scholarship than to environmental politics and policy. To be sure, Grundmann is much more modest in his claims than is often the case in Marxist scholarship: "If I say that Marx's framework is still illuminating this does not mean that it gives us a point of reference which is sufficient for the understanding of ecological problems" (p. 6). Still, the connections Grundmann makes between his analysis of technology, social evolution, and ecological problems are flawed by his limited understanding of ecology, environmentalism, and the deeper theoretical levels of the interdisciplinary environmental critique. Indeed, chapter 1 on ecological problems reads as though it were tacked on to justify the title of the book, since Grundmann rarely links the main body of his argument to ecological problems but presents his arguments about technology and historical materialism as significant in their own right (which perhaps they are). Their relevance for ecological problems is less apparent, however, since the claims he makes seem dubious in light of, for example, evidence of considerable ignorance of the science of ecology (he uses "ecology" exclusively to refer to the social and political environmental, or green, movement). Furthermore, Grundmann dates back the contribution of social science to environmental understanding only 15 years. A considerable discourse on environmental philosophy since 1974, particularly theorizing about ecocentrism and anthropocentrism, is overlooked; and he is unable to identify any outstanding contributions to the theoretical debate from sociologists or political scientists (p. 56). Much of the environmental problematique is trivialized by being neatly defined away as merely resource problems (p. 13). Population growth is likewise dismissed (p. 65).

Neither *Marxism and Ecology* nor *Commons without Tragedy*, then, are seminal works in environmental politics research. Both are seriously flawed, but parts may be of special interest to some theorists. *Marxism and Ecology* may be, in addition, of significant interest to Marxist scholars, as well as researchers grappling with concepts in the politics of technology.

Purdue University ROBERT V. BARTLETT

New Philosophy of Social Science: Problems of Indeterminacy. By James Bohman. Cambridge: Massachusetts Institute of Technology Press, 1992. 273p. $32.50.

What's new in our understanding of social science, according to *New Philosophy of Social Science*? The book's subtitle provides a clue. Bohman thinks "[that] social phenomena are shot through with indeterminacy and open-endedness, and that good explanations in vital research programs must find ways to deal with the problems that this indeterminacy raises" (vi–vii). It is not easy to discover precisely what Bohman means by *indeterminacy*. Sometimes it characterizes *social phenomena*, a feature deriving from social agents' capacity to alter their circumstances on the basis of reflection. At other times, it distinguishes *explanations* of social actions: "If agents become aware of and change the conditions under which they act, no factors or set of factors can fully or determinately explain a social event or action" (p. 13). Bohman assumes that indeterminacy of agency implies indeterminacy of explanation. (Though this is plausible, it is by no means obvious. A number of research programs in the social sciences deny it. He might well have provided more argument in support of this assumption.)

Bohman also thinks that indeterminacy is a feature of social science itself. He believes that "postempiricism" requires abandoning the belief in necessary-and-sufficient conditions of science, of explanation, and of interpretation. All science must be understood in "antiessentialist" and historicist terms. Only particular explanatory strategies for particular *explananda* exist, each of which must be examined and assessed in its own particular historical and explanatory terms. Much of the book is devoted to detailed expositions and criticisms of four research strategies (rational choice theory, ethnomethodology, interpretive social science, and Habermas' theory of communicative action) and their attempt to explain social actions and rules (chap. 2), social meanings (chap. 3), and macrosociological phenomena (chap. 4), as well as their capacity to support social criticism (chap. 5). These analyses are unfailingly well done; readers seeking state-of-the-art discussions of these topics need search no further than this book.

An obvious difficulty with postempiricism is how to provide standards of assessment by which to evaluate various particular explanatory strategies if no overarching criteria exist. It is no accident that relativism has plagued postempiricism. But though he embraces postempiricism, Bohman adamantly rejects relativism. Indeed, he defines one of his central tasks as "developing forms of rational assessment for the explanatory power of theories produced by various ongoing research programs" (p. 4).

A tension exists at the heart of Bohman's attempt to provide nonrelativist but nonessentialist criteria of rational assessment. This tension is rooted in his commitment to both a social science of indeterminacy and postempiricism. Bohman claims that social science explanations must be indeterminate; it is on this basis that he develops his criteria of theory assessment. But surely, this is an essentialist claim about social science. Is this not a case of denying the possibility of necessary general features, employing them all the while?

Bohman's implicit reply to this sort of criticism is, "Don't get hung up on general epistemological questions. Instead, look at the actual theories. Evaluate them in their own terms. Assess them relative to other theories when their explanatory aims are similar enough." If one does this, Bohman suggests, one will see that it is only the theories that recognize indeterminacy, agency, open-endedness, and complexity that are successful.

This may be true, but it is insufficient to support Bohman's general claims about indeterminacy. Just because nonindeterminate research strategies have been unsuccessful is no reason whatsoever to abandon them. Those who explicitly deny the agency-centered approach favored by Bohman (call them naturalists) may well have the last say. Who can say, a priori?

In fact, the postempiricist side of Bohman must agree. Postempiricism shuns a priori legislating to practicing social scientists. It eschews the kind of Procrustean simplicity often found in philosophies of social science. Thus, the most Bohman can say is that at the moment only indeterminacy-centered approaches work and seem the most promising for development; but he must admit that others—even the radically different, naturalistic approaches—may forge ahead in the future.

A last question arises, however, about the sort of assessment Bohman practices: Is it in any interesting way philosophical? Bohman writes that philosophical reflection

> should take up a comparative but no less normative role judging research programs and their typical explanations to be better in some respects and worse in others. While such comparative criteria of evaluation are not strong enough for many epistemologies, they are sufficiently normative for the social sciences and the practical knowledge they warrant. (p. 14)

But is such evaluation not a job for social scientists themselves? Indeed, do social scientists not engage in such criticism as part of their ongoing practice all the time? The odd result of *New Philosophy of Social Science* is that in its postempiricist concern to avoid a priori, universalistic abstraction, it ends up denying any special role for philosophical reflection about the social sciences. Some readers of this journal will be pleased with this result, which justifies their ignoring philosophy altogether. But others will find this worrisome, feeling either adrift without general guidelines or hypocritical as they employ such guidelines (as Bohman does when he invokes indeterminacy as the touchstone for evaluation) without any general warrant for so doing.

Wesleyan University Brian Fay

Civil Society and Political Theory. By Jean L. Cohen and Andrew Arato. Cambridge: Massachusetts Institute of Technology Press, 1992. 771p. $45.00.

Any critical theory articulates and analyzes the goals and activities of oppositional social movements—what Marx called "the struggles and aspirations of the age." As Cohen and Arato point out, even cursory observation of such movements across the globe—from Eastern European movements against state socialism, to Latin American attempts at liberation from authoritarianism, to European and North American ecology and feminist movements—shows that the concept of *civil society* plays an increasingly important role in such an analysis. Not only do movement participants often use this term in their own discourse, but it also captures how the aims of these movements clearly contrast with previous emancipatory, revolutionary social movements. Rather than seek total transformation of society, such movements propose, in the words of Jacek Kuron, "self-limiting revolutions." Their goal is neither total social transformation nor revolutionary rupture but, rather, "the construction from below of a highly articulated, organized, autonomous, and mobilizable civil society" (p. 32). Nonetheless, civil society remains an overly broad practical orientation and a vague theoretical term. For this reason, Cohen and Arato propose a systematic, normative reconstruction that shows why civil society is the "terrain and target" of struggles for democratization in contemporary societies. Their book provides the most comprehensive and theoretically sophisticated analysis of this category in the literature to date.

Perhaps Cohen and Arato's main contribution is to set forth with clarity the contours of a "post-Marxist," yet critical, social and political theory of democracy (p. 71). Here they follow the recent work of Jurgen Habermas and call their reconstruction of civil society a "political 'translation' of Habermasian critical theory" and discourse ethics (p. xvii). In social theory, Cohen and Arato argue that civil society, understood as a "sphere of interaction between society and the state," is required to do justice to the achievements of differentiation and plurality in modern social institutions. Social utopias of "de-differentiation" can be avoided by a "three-level" theory of society, where civil society occupies the position between the economy and the state. In political theory, the term *civil society*, understood as the sphere of voluntary associations and public communication, is necessary to avoid the dangers of radical democratic "fundamentalism" and apologetic liberalism. By understanding civil society, critical theorists can overcome the previous utopian hopes of a state without power, well-being without industrial production or public administration, and democratic participation without individual rights or the constitutional state. Moreover, the concept of civil society contains a new utopian goal of a "plurality of democracies," a vibrant Tocquevillian "self-limiting radical democracy" consisting of voluntary associations, social movements, a formal parliamentary political system and a public sphere of free communication.

Cohen and Arato develop this reconstruction in three parts. In part 1 they show the centrality of the concept of civil society in various theoretical debates and in the discourse of various political movements. They situate their theory of civil society within the tradition of social and political theory stemming from Hegel, Tocqueville, and Parsons. In part 2 they use the critics of civil society (including Arendt, Schmitt, Foucault, the early Habermas, and Luhmann) dialectically to develop their own conception further and to show the inadequacy of various versions of the "fusion" of civil society into the state or the economy. The treatment of each of these critics in detail adds to the considerable length of the book. In part 3, they fully develop their reconstruction based on the emergence of a civil society between the state and the economy which entails plurality, publicity, legality, and privacy (p. 346). These structures not only secure a modern differentiated civil society but also permit a self-referential communicative practice that renews, develops, and limits civil society all at the same time, below the level of formal organization of the state and its direct political power. In perhaps the most original aspect of this reconstruction, "new social movements" are shown to engage in the "dual politics" that is based in such communicative practices. They are "offensive" in influencing political and economic policy, especially in the direction of greater inclusion; they are "defensive" in self-reflexively "preserving and developing the communicative infrastructure of cultural and associational life out of which new identities can be created" (pp. 531–32). Such associations, publics, and legal rights represent the boundary conditions for a civil society "within whose limits much of the program of radical democracy can be reformulated" (p. 474).

Cohen and Arato believe that the political changes in the direction of such a self-limiting democracy are now taking shape and that for this reason the precise character of such a politics is still being worked out historically. Two questions remain even after their extensive reconstruction. First, are differentiation and plurality in civil society always directly linked to the democratization of society? Both could simply increase complexity but not democracy. Cohen and Arato place the public sphere within civil society (p. 396). But it seems that democratization is always mediated by the public sphere, whether formal or informal, and thus that a differentiated and plural civil society is only indirectly a condition of the possibility of democratization insofar as it makes possible a vibrant public sphere. Second, the notion of self-limitation clearly contrasts with overly utopian and monolithic conceptions of democracy that Cohen and Arato rightly criticize. But how does self-limitation work in civil society itself and in its relations to other social systems? Conceptions of self-limitation such as "precommitment" models do not work here; and it remains to be described exactly how and by what mechanisms the communicative practice of civil society is self-limiting. Is it only by the coercive power of law that backs up rights, which the paradigm case of internal democratic self-limitation in this analysis? If so, how does self-limitation work with regard to other spheres, such as markets or the state itself, systems that do not have self-referential, limiting mechanisms? Cohen and Arato's answer is that civil society does not regulate them but influences them first and foremost by transforming itself. After articulating such a compelling theory of civil society, Cohen and Arato's next contribution could be to develop a more complete account of self-limiting democratic change within it.

St. Louis University JAMES BOHMAN

Local Justice: How Institutions Allocate Scarce Goods and Necessary Burdens. By Jon Elster. New York: Russell Sage Foundation, 1992. 283p. $24.95.

If the allocation and distribution of goods and burdens is central to both politics and economics, then *Local Justice* should cover some crucial issues in political economy. The topic is narrowed somewhat by Elster's focus on scarce and mostly indivisible goods and burdens as allocated in kind by purposeful and autonomous governmental bodies to identifiable individuals. The problems covered therefore include human organs, military service, (French) sperm, college admissions, adoption, child custody, immigration, employment layoffs, and wartime rationing. Obviously, a lot of scarce goods and burdens are excluded by this definition—as are goods that do not involve scarcity, such as places in heaven as allocated by God (p. 26; Elster seems unaware of Christian sects that put numerical limits on this good). Justice is local in the sense that it is confined to the particular good or burden as allocated by a particular autonomous institution, be it a draft board, court, or admissions office.

Variety in local justice problems is matched by a proliferation of principles, rules, and mechanisms for allocation. The latter covers everything from lotteries and points systems to seniority and professional discretion. Rarely can any particular mechanism prove superior on all criteria in a particular case—let alone across all cases. Schemes that try to incorporate multiple principles can become baroque, if not bizarre. The world of local justice according to Elster is a diverse and complicated place—hardly, it would seem, a single world at all. But Elster strives valiantly to impose some order on this chaos by categorizing allocation principles, looking at the consequences of particular principles and combinations, and explaining why principles and mechanisms are adopted. Along the way, the reader is sensitized to the often-perverse incentive effects of seemingly justifiable principles. (Individuals can go to great lengths to affect their eligibility, such as having children or removing a body part to avoid the draft.) The explanations for why principles are adopted stress public opinion, professional norms, national characteristics, and political mechanisms for preference aggregation.

When all is said and done, few generalizations across cases prove possible, still less any general theory of local justice; and Elster consciously retreats from this objective. Though in several places he brings his customary analytical acuity to bear, the analysis and discussion (and thus the book) prove radically particularistic. Nevertheless, Elster provides all kinds of useful insights into what he refers to as the phenomenology of local justice (p. 17). The reader learns a great deal about the sheer variety of ways to allocate goods and burdens, from the complex ways children are tested for eligibility for admission to Norwegian kindergartens to the ingenious methods college admissions officers in the United States have used to limit the enrollments of particular ethnic groups.

By the end of this exercise, the reader also has a feel for the general inapplicability of general theories of justice as advanced by moral philosophers. The usual suspects—utilitarianism, Rawls, and Nozick—make an appearance in chapter 6. Despite the occasional parallel that can be drawn between a local justice practice and these philosophical principles, the more frequent case seems to involve substantial divergence. Elster goes on to induce (somewhat impressionistically) a "commonsense" theory of justice held by most decision makers, involving maximization of total welfare, subject to a democratically defined floor for individual welfare and excluding people who drop beneath it through their own informed choice.

The articulation of this commonsense principle is the closest Elster comes to a conclusion, and the final chapter contains only an agenda for future research. The lack of any more general concluding statement may, in fairness, be partly a function of the subject matter. *Local Justice* remains an instructive volume which should be remembered as one of the better books published by Jon Elster in 1992.

University of Oregon JOHN S. DRYZEK

Benjamin Constant and the Post-Revolutionary Mind. By Biancamaria Fontana. New Haven: Yale University Press, 1991. 165p. $25.00.

The Humane Comedy: Constant, Tocqueville, and French Liberalism. By George Armstrong Kelly. New York: Cambridge University Press, 1992. 262p. $59.95.

Liberty/Liberté: The American and French Experiences. Edited by Joseph Klaits and Michael H. Haltzel. Baltimore: Johns Hopkins University Press, 1991. 218p. $35.00.

Liberalism is very familiar to the English-speaking academic world only in its Anglo-American form. Not surprisingly, our understanding of liberalism reflects the biases inherent in Anglo-American political and philosophical thought, with its traditional preference for methodological individualism, legal and political (as distinct from social) theory, and negative (over positive) liberty. As such, liberalism has been roundly criticized for its sociological weakness. When directed against liberalism so conceived, these criticisms have considerable force. However the French liberal tradition (particularly in the nineteenth century) cannot be similarly impugned, since it is sociologically very rich indeed. The study of French liberal thought is therefore very instructive in the context of contemporary debates about the nature of liberalism, and provides it with many more resources than its Anglo-American cousin in answering some of the challenges that have characteristically been directed against it.

Benjamin Constant (1767–1830) is among the most important figures in French liberal thought. Yet he occupies an uncertain position in the history of ideas. Although his name is familiar, he is little read and only occasionally referred to. Yet he was among the first thinkers to attempt to reformulate liberal theory in the wake of the French Revolution. Biancamaria Fontana's new study of Constant takes the view that he has been seriously underestimated and that his failure to become "a popular and charismatic thinker" should therefore be reexamined. As a moderate in an age of extremes, she argues, Constant could not find a place "within a historiography which carried well into our century the ideological conflicts and division of the revolution of 1789" (p. xii). She believes that the study of his life and work offers a "unique opportunity to approach the political tradition with which his name is currently associated—modern liberalism—by looking beyond ideological stereotypes to confront the new private and public sensibility of the age" (pp. ix–x).

Although Fontana believes that Constant has been "much underestimated" (p. ix), she does not really explore in detail why this is so. However, one explanation seems to be implicit in her study, which abounds with words such as *hazy, vague, uneven, incomplete, uncertain, circuitous, repetitious, restless, diffuse, fragmentary,* and *ambiguous.* The reader is left with the impression that Constant, while frequently insightful, was often an extremely vague and uncertain thinker. As Fontana herself points out, he harbored a deep suspicion of ambitious philosophical systems. Despite this fact, he took it upon himself to construct a new political idiom and principles appropriate to the age midwifed by the French Revolution. Yet Constant eventually abandoned the major political treatise that he began working on at the turn of the century. Although he has left us with drafts of this work, they are incomplete and underdeveloped in many ways. And despite laboring over his mammoth work on religion for over 40 years, he was not able to finish this study either, which was similarly plagued by the author's vagueness and deep distrust of systematization. It is difficult to avoid sharing Constant's ultimate dissatisfaction with his own work.

Fontana's book is a relatively short intellectual biography of the nineteenth-century Swiss writer. She states at the outset, however, that it is not intended to be an "exhaustive illustration of Constant's life and work" (p. xvii). The mode of the book is exegetical, rather than critical. It provides a good overview of Constant's mind and character, with a particularly careful and lucid exposition of his political ideas. The author also displays considerable sensitivity to the uniqueness and magnitude of Constant's historical and intellectual predicament as a thinker attempting to restore political theory in the decimated world of postrevolutionary Europe. Fontana's book emphasizes the uncertainty and ambiguity that pervades Constant's life and work, leaving an impression of him as an almost tragic figure: pessimistic, lonely, and restless in an age of unprecedented doubt and skepticism. The most revealing image of Constant, Fontana argues, is that of a solitary traveler on a horse, "wandering, with open eyes, in the shapeless landscape which has become our present" (p. xvii).

While it is undoubtedly true that Constant was much more skeptical of progress than his Enlightenment predecessors and had a heightened awareness of the dangers that threaten liberal society as a result of the French Revolution and Napoleon, he was nonetheless quite sanguine about the general course of historical progress. For example, he wrote in the *Mércure de France* in 1817 that "our present governments have today many fewer dangers to fear than ever before." These are hardly the sentiments of a skeptical pessimist. And when compared to the other great mind of nineteenth-century French liberalism, Alexis de Tocqueville, Constant emerges as virtually Panglossian.

Just such a systematic contrast between Constant and Tocqueville (1805–59) is provided in George Armstrong Kelly's book on French liberalism, *The Humane Comedy.* In this study, Constant emerges as a cautiously optimistic liberal, a somewhat muted echo of his enlightenment predecessors devoid of the ennui attributed to him by Fontana. Tocqueville's darker vision is depicted as an anticipation of French liberalism's subsequent disillusionment and eventual "Parnassian" retreat from active politics. Next to Tocqueville, Kelly argues, "Constant hardly saw or projected the problems of modern liberty in democratic ages" (p. 46). He concludes that Constant's vigilance is "directed against the past," whereas Tocqueville "posts his sentinels for time to come" (p. 41). This contrasts sharply with Fontana's depiction of Constant, which ends with the observation that "the only relatively clear image of a possible historical future to be conjured up in Constant's writings appeared in the form of a nightmare" (p. 135). In other words, we have two quite different Constants here. Fontana's portrayal shows him to be an almost Tocquevillian pessimist, the anguished voice of the postrevolutionary mind, whereas Kelly's stresses his divergence from Tocqueville's pessimism. Although there are elements of both optimism and pessimism in the writings of Constant, I think that Kelly's is the more convincing depiction. Constant was born and educated (at the University of Edinburgh) during the Enlightenment, much of which he absorbed and carried into the nineteenth century. Tocqueville, on the other hand, was still an adolescent at the time of the Restoration. Unlike Constant (who was almost 40 years his senior), Tocqueville was to live through the Revolution of 1848, by which time the fragility of liberalism in France was much more apparent. Kelly's book also provides a broad and nuanced comparative context within which to situate Constant in the history of French liberal thought. His comparison of Constant and Mme. de Staël is particularly illuminating. And he uses Tocqueville (who, he points out, deliberately distanced himself from Constant) very effectively as a foil to highlight some of Constant's weaknesses.

After exploring the complex relationship between the ideas of Constant and Tocqueville, Kelly examines a host of other, more obscure (in some cases, forgotten) figures in nineteenth-century French liberal thought. He discusses the lives and ideas of Mme. de Staël, Necker, Guizot, Royer-Collard, Rémusat, Cousin, Lamartine, Laboulaye, Lacordaire, Jouffroy, Rénan, Montalembert, and Flaubert. And Kelly constantly returns to Constant and Tocqueville as points of reference and comparison when presenting the ideas of these other thinkers.

This book, while regrettably incomplete at the time of the author's death, is nonetheless a superb piece of intellectual craftsmanship. It is extremely rich in historical and biographical detail and balances contextual sensitivity and analytical rigor brilliantly. Kelly is particularly adept at identifying and tracing the subtle intellectual connections between, and influences upon, the thinkers whom he is discussing. This book is a model of the art of writing intellectual history.

In addition to analyzing the fissures within nineteenth-century French liberalism, Professor Kelly has explored the complex relationship between liberalism and Jacobinism in the Second and Third French Republics in an excellent essay in the recently published collection, *Liberty/Liberté,* edited by Joseph Klaits and Michael Haltzel. It consists of 14 essays that either compare the origins and development of the idea of liberty in France and the United States or examine the relationship between liberalism and other political ideologies, particularly democracy (in the case of the United States) and Jacobinism (in the case of France).

The quality and originality of the essays in this collection are quite uneven. At one extreme is Kelly's piece, which is entirely consistent with the style and depth of his book. At the other extreme, however, is the essay by Michael Novak. This piece, which "summarizes the neoconservative rationale for free-market economic relations" (p. 6), is unscholarly in tone and extremely familiar in content. It is not at all apparent how this essay fits into the main themes of the book. This collection also contains several interesting discussions of Tocqueville, whose ideas inevitably figure in studies of the idea of liberty in its American and French contexts.

Balliol College, Oxford GRAEME GARRARD

Gramsci's Democratic Theory: Contributions to a Postliberal Democracy. By Sue Golding. Toronto: University of Toronto Press, 1992. 221p. $45.00 cloth, $17.95 paper.

Ever since the publication of *Selections from the Prison Notebooks* in 1971, the conceptual and methodological ruminations of Antonio Gramsci have attracted the attention of scholars in a variety of disciplines in the social sciences and humanities. Now, with the ongoing publication of his entire notebooks in a multivolume set by Columbia University Press, his work should have an even greater resonance in the areas of cultural studies, social history, literary criticism, anthropology, and even political science.

Apace with his oeuvre is an ever-burgeoning literature on his life and ideas. The overwhelming majority of critical assessments of Gramsci's journalism and prison writings have focused upon his now classic innovations: historical bloc; wars of position and maneuver; and, of course, hegemony. His criticisms of economic determinism and emphasis on cultural, political, and ethical elements of social reality have had profound implications for Marxist thought and social theory in general.

One less explored dimension of his work, however, is his concern for the development of a proletarian-based state and civil society that avoided both the authoritarian leanings of a Leninist model and the reproduction of socioeconomic inequalities under a liberal–capitalist version. This concern led Gramsci to thinkers as diverse as Sorel, Bergson, Vico, Croce, and Hegel to enrich his own understanding and application of Marxism. Sue Golding's *Gramsci's Democratic Theory* explores this dimension of Gramsci's writings in order to underscore the Sardinian intellectual's contribution to postliberal theories of democracy.

While several notable scholars have focused upon the role of contingency, human diversity, and agency in Gramsci's "philosophy of praxis," Golding amplifies this recognition by attributing Gramsci's more fluid reading of Marxism to his preoccupation with "historical immanence," a notion of ethical improvement that was grounded in historical reality and circumstance. For her, Gramsci's desire to craft the "ought-to-be" from "what is" represents his transcendence of the more benign and ultimately apolitical formulations of ethics by thinkers such as Kant, Croce, and Hegel. Golding believes that in doing so, Gramsci avoids the scientistic and utopic visions of both Marxist and liberal thinkers. Gramsci's philosophy of praxis then, is a combination of an historicized ethical imperative, humanist principles, and a materialist conception of history.

At the same time, Golding covers more familiar Gramscian territory, with chapters on base and superstructure and the central role that politics, not relations of production, plays in Gramsci's strategies for reformation and revolution. This ground is already covered in Joseph Femia's *Gramsci's Political Thought* (1987), Anne Showstack Sassoon's *Gramsci's Politics* (1980), and Ernesto Laclau and Chantal Mouffe's *Hegemony and Socialist Strategy* (1985). What is new about Golding's treatment are her considerations of Gramsci's epistemological eclecticism in chapter 2 and the importance of the concept of the will in chapter 4, which should be most helpful for those interested in exploring the philosophical geneaology of Gramsci's Marxism.

Golding accentuates the philosophical dimensions of Gramsci but, in so doing, often obscures the role of praxis. This is my only criticism of this substantive, provocative work. By the book's end, it was not clear to me what *precisely* Gramsci's connection to postliberal democracy and theory is (particularly the work of C. B. Macpherson, mentioned in chapter 1), in part because the term *postliberal* can convey the same nebulous associations as another well worn phrase, *postmodernism*. How one defines and extends notions of a postliberal democracy during an era in which the very tenets of liberalism are under siege in both eastern and western Europe is as much an issue of "common sense"—the politics of the everyday—as it is of philosophy, which Gramsci referred to as "good sense." Just as Gramsci attempted to reinvigorate Marxist thought in response to fascism's rise, so must postliberal democratic theory confront the crisis of liberalism *and* the rise of intolerance and particularist forms of inequality in the 1990s. This is not a task of Golding's alone, however, but of theorists and political actors more generally. Sue Golding has made a useful, thoughtful contribution toward this end.

University of Texas, Austin MICHAEL HANCHARD

Privacy, Intimacy, and Isolation. By Julie C. Inness. New York: Oxford University Press, 1992. 157p. $24.95.

Before the arrival of Inness's attempt to reconceptualize the "chaotic world of privacy," scholars of a similar commitment had pretty much focused on *action* they deemed invasive. Now, with Inness's entry in the growing bibliography on privacy, *autonomy* becomes as important as simple, intrusive actions against the individual. Early on, Inness notes correctly that privacy law and those who write about it have failed to produce a unified account (much less a unified theory) of privacy. Law, she concludes, is not the route to resolving privacy's chaotic state. But philosophers are in no better agreement about the conceptual distinctness of privacy than are the juridical folks. One group of philosophers places the regulation of information at the heart of its definitions of privacy. Another group opts, instead, for definitions based on noninformation: seclusion, solitude, anonymity. The task Inness sets for herself is to disentangle, if not sometimes denude, the concepts and notions of other philosophers, then develop a theory of privacy that is grounded in intimacy: "We value being respected as autonomous emotional agents who can form their own life plans with respect to intimacy. Hence, privacy protects us from both manipulative and paternalistic intervention into our zone of autonomy with respect to intimacy."

Privacy claims that rely nearly solely on property rights are especially troublesome, for justified property claims may not always entail justified privacy claims. The fact that nonintimate possessions are deemed private property, Inness argues, does not suggest that privacy claims regarding those possessions are necessarily justified: "If a friend examines my pen sitting on my desk, she usually does not violate my privacy despite the fact that it is *my* pen, but if she examines my open diary on the desk, a privacy violation occurs." Thus, in Inness's scheme, privacy claims are not indiscriminately attached to the simple fact of ownership, whether of property per se or of information about oneself. In any case, the determining factor is "intimate aspects of the person," according to Inness.

Inness analizes the rift between separation-based and control-based definitions of privacy, the former deforming, and the latter capturing, the nature and value of privacy. A separation-based view says that privacy is the state of not being looked at or listened to, suggesting that "shared privacy" is impossible. Inness, the syllogist, writes "Assuming that lost privacy is inherently undesirable, and that lessening our separation from others is not necessarily undesirable, it follows that lessening our separation from others is not always a loss of privacy." In other words, publicity is not necessarily opposed to privacy.

Control-based accounts of privacy, on the other hand, give the *individual* control over a certain intimate area of *personal* life and secure, in the process, a specified realm of autonomy: "Given the modern liberal assumption that autonomy is a positively valued condition, control-based definitions . . . incorporate an underlying assumption that privacy is positively valued." Any curtailment of the individual's control over the private domain must be justified. The justification must be strong enough to overcome the positive value liberalism places on autonomy.

As for the content of privacy, that is, the information about, access to, or intimate decisions about our actions, Inness defines privacy as "the state of the agent having control over a realm of intimacy, which contains her decisions about intimate access to herself (including intimate information access) and her decisions about her own intimate actions." This follows Alan Westin's popular explanation that privacy is "the claim of individuals, groups, or institutions to determine for themselves when, how, and to what extent information about them is communicated to others." Yet here, too, Inness allows that intimacy, not information, is the defining term—that some, but not all, information must be protected by privacy. "We must look at the *type* of information disseminated; it is the *intimacy* of this information that identifies a loss of privacy."

The content of privacy includes our decisions about intimate informational access, intimate access, intimate actions, and (most important) intimate decisions—which involve, in Inness's model, "a choice on the agent's part about how to (or not to) embody her love, liking, or care." Inness assumes that since society values respecting agents as "persons with the capacity to love, care, and like," it must therefore value protecting the autonomy of these agents with respect to these emotions. Behavior alone, however, does not constitute the source of intimacy, which stems from something prior to behavior.

Readers who have not spent equivalent time with the same legal and philosophical concerns will find Inness's treatise interesting and challenging. Others not so patient may find the work unnecessarily tedious—much of it reads like unedited lectures. Either way, the work must be taken seriously by scholars and policymakers alike.

Rutgers University RICHARD F. HIXSON

The Principle of Fairness and Political Obligation. By George Klosko. Savage, MD: Rowman & Littlefield, 1992. 204p. $52.50 cloth, $19.95 paper.

Klosko makes a valuable and thought-provoking contribution to liberal theory by providing detailed liberal argument for political obligation that is based on the principle of fairness (or fair play) introduced in Hart's

(1955) "Are There Any Natural Rights?" Klosko notes that "it is now widely doubted that a defensible account of political obligation can be constructed on liberal premises" (p. 21) and attempts to belie this view. In doing so, he makes very good use of both historical and contemporary texts, paying particular attention to two works skeptical of the possibility of a liberal account of political obligation, namely, Carole Pateman's *Problem of Political Obligation* (1979) and A. John Simmons's *Moral Principles and Political Obligations* (1979).

Klosko delineates five major requirements of any acceptable theory of political obligation. The theory must (1) be suitably "general," establishing the obligation of "all or most members of society"; (2) ground obligations that are "limited," thereby avoiding illiberal absolute governments; (3) explain people's obligation to support a suitably wide "range" of governmental activities; (4) account for the "particularity" of political obligations, that is, the fact that people consider themselves to be obligated to just that particular state with which they have a special relationship; and (5) "explain the obligations that people have to existing governments" (pp. 3–5).

In arguing that the principle of fairness can justify political obligations, Klosko employs a number of liberal assumptions, one of the most important of which is entailed in requirement 5, namely, that there are currently many people subject to legitimate political obligations. This is consistent with his adoption of the "coherence method" (Brandt's "standard method" or Rawls's "reflective equilibrium") according to which he can comfortably rely on the five requirements cited because they "express some of our deepest intuitions about political matters" (p. 22). Thus, Klosko counters Simmons' argument for philosophical anarchism by rejecting the appeal to false consciousness with respect to existing political obligations upon which Simmons must rely. Klosko may be too quick, however, to reject the combining of the coherence method with claims about false consciousness, since, as he notes, this move is justified if one "can clearly distinguish between our convictions in general and some class of them that are suspect" (p. 25). A supporter of Simmons could make just such a distinction, arguing that the belief in existing political obligations cannot function as just one more independent axiom of our political argument, because when this axiom is taken together with our other commitments, a contradiction arises; and this is the only single axiom whose removal eliminates the contradiction.

After establishing his approach in the first chapter, Klosko dedicates the central four chapters to a strong presentation of the ability of the principle of fairness to ground a theory of political obligations that meets the five cited requirements. Klosko's central claim is that people have an obligation grounded in the principle of fairness to contribute to a nonexcludable scheme, that is, a scheme for which nonparticipants cannot be excluded from the benefits if it provides them with "presumptively beneficial goods," that is, goods that are "indispensable to acceptable lives" (p. 40). It is clear how this claim addresses the five requirements, with the exception of requirement 3, namely, that people have an obligation to support a wide range of governmental activities. Klosko suggests both that many discretionary public goods are actually indirectly necessary for the provision of presumptive public goods and that one ought to accept a wide range of governmental impositions so as not to undermine the effectiveness of the institution that provides presumptive public goods.

Clearly, any theory of political obligation must meet Klosko's first requirement; it must apply to "all or most members of society." Klosko suggests that the principle of fairness generates political obligations in a manner that avoids the free-rider problem because it would be unfair for anyone not to contribute to a system that provides something indispensable. Exactly how the provision of something indispensable generates an obligation, however, is a crucial issue. Klosko claims that an individual's obligation "does not stem from hypothetical consent, but from the fact that he receives the benefits" (p. 42). Unfortunately, Klosko does not directly support his admittedly plausible theory in contrast to the theory of hypothetical consent. Furthermore, he employs arguments that read very much like hypothetical consent theory:

> Because the benefits of national defense are presumptively beneficial, we can assume that Pickerel *would* pursue them (and bear the associated costs) if this were necessary for their receipt. If we imagine an artificial choice situation analogous to a state of nature, it seems clear that under almost all circumstances Pickerel would choose to receive the prescribed benefits at the prescribed cost, if he had the choice. Because of the indispensability of national defense, it would not be rational for him to choose otherwise (pp. 41–42).

Shortly after, he claims that "because the benefits are indispensable, he could not say that he does not want them" (p. 42).

After completing his discussion of how the principle of fairness generates political obligations that meet the five requirements, Klosko provides a concluding chapter in which he explicitly addresses the shortcomings of the principle alternative approaches to justifying political obligations: utilitarianism and consent theory. Since hypothetical consent theory is not subject to his general criticism of consent theory, which focuses on the fact that actual consent (either express or tacit) is rarely given, he addresses it in a note. He claims that "it is not clear how the fact that A would consent to government X under various circumstances that do not obtain gives rise to an obligation to X *that is based on consent* under existing circumstances" (p. 144, n. 39). It is not obvious, however, that hypothetical consent theory entails fundamentally different reasoning from Klosko's employment of counterfactuals in arguing that the benefits provided by presumptive public goods are of the sort that generate obligations of fairness. Klosko not only employs counterfactual reasoning to suggest that the recipient considers the benefits suitably indispensable and thus worth a fair contribution but also, like a hypothetical consent theorist, he claims that one who thought otherwise "would not be rational."

Anyone interested in the problem of political obligation will be stimulated by this book, which provides a good sustained argument for a plausible account of political obligation that is worthy of careful consideration.

University of Texas, Austin DONALD BECKER

Modern Political Theory and Contemporary Feminism: A Dialectical Analysis. By Jennifer Ring. Albany: State University of New York Press, 1991. 229p. $49.50 cloth, $16.95 paper.

Jennifer Ring's project is to "salvage" traditional conceptualizations of subjectivity and objectivity in a form compatible with feminist theorizing (p. 19). Through a discussion of J. S. Mill, Marx, and Hegel she proposes a *minimalist dialectics,* which she defines as "nothing more than a focus upon that with which feminist theory seems most uncomfortable: conflict" (p. 48). In her view, feminism's most pressing need is for method, rather than substance, and the most appropriate philosophical structure for feminism can be derived from traditional theories of the dialectic.

Ring criticizes J. S. Mill's pretentious objectivity and hidden but self-righteous subjectivity as it emerges in *System of Logic* and *The Subjection of Women.* Rejecting his liberal empiricism as logically inconsistent in methodology and epistemologically static, she turns to a discussion of the materialist and idealist dialectics of Marx and Hegel. Why? "If nothing else, it may be useful in conserving effort and preventing the further isolation of feminists, an isolation that is driving them to an attempt to rewrite all philosophy and metaphysics specifically for feminist theory" (p. 105). Such a rewriting of the canon is viewed as terribly misguided.

Ring believes it is possible to liberate the structure of dialectics from the sexist substance of Hegelian and Marxist thought. Once the form is purged of its unfortunate sexist content, minimalist dialectics will be an appropriate methodology for the elaboration of a feminist epistemology. It is minimalist because it is concerned not with the origins and ends of history but with specific times and frames of being. It is dialectical, in Ring's view, because it insists on the necessity of conflict in achieving knowledge and intimacy. Yet her separation of structure and substance is somewhat idealist and lacks an understanding of the meaning of masculine dominance in the production of knowledge.

One of the greatest difficulties with the book is the sense in which Ring seems to view dialectics as a lovers' discourse (more accurately, a lovers' encounter and dispute). In Hegelian subjectivity and objectivity she sees not dominance or violence but a necessary and essential paradigm of risk and surrender. Ring speaks of dialectics as "falling in love," with various stages of infatuation, idealization, destruction of ego boundaries and the final creation of "a love that can survive in the world" (p. 125). Yet such (hetero)sexist assumptions about universal stages of desire and essential needs for surrender cannot be "corrective" of feminist theory. Indeed, feminist theory has indicated the failure of such methodologies.

Ring criticizes Evelyn Fox Keller for not fully accepting the "importance of discord and negation fully experienced": only a complete openness to annihilation moves us toward the truth and "Keller is afraid to let go" (pp. 42–43). Nancy Hartsock's compelling critique of erotic brutality, death, and abstraction is silenced with "Is Hartsock running from an aspect of life?" (p. 47). I find this to be a romantic and reconciled, not a feminist, text when it asks us to "forego the political prejudice that associates 'objectification' with abuse" and adopt the Masters' structure (p. 22). In order to reconsider objectivity, Ring's analysis should have engaged feminist readings of reason and dialectics, such as Pat Mills's (1987) discussion of Marx and Hegel in *Woman, Nature, Psyche* and (especially) Michele Le Doeuff's (1989) *Philosophical Imaginary,* which raises critical questions about the relation of women to philosophy, and erotic/theoretical "encounters."

Ring reduces and simplifies diverse feminist theory to a "prevailing feminist contention" (p. 1) that dualism and conflict are male constructs. The work of Alison Jagger, Sandra Harding, and Evelyn Fox Keller lacks power, according to Ring, because these feminists overemphasize unity and the harmony between subject and object. Mary O'Brien's contention that Hegel and Marx appropriated dialectical structure from reproductive process is dismissed as a somewhat trivial theory of reproduction. In contrast, Ring wants to "use dialectics as a basis for exploring the compatibility of male and female experience" (p. 45). Carol Gilligan is distorted as writing about innate nature, rather than processes of object relations and contemporary child-rearing practices. Sara Ruddick's "confused thinking" could be solved by consulting "a biologist or a geneticist, to find out about the sources of aggression or the lack of it in hormones, genes, etc." (pp. 16–17). Indeed, while she takes feminist theory to task for its approach to difference, it is Ring who remains tied to a certain deterministic—indeed, Darwinian—notion of nature, survival, and evolutionary conflict.

Ring's impulse is fundamentally conservative: she adheres to traditional progress and the progressive tradition. She refuses to start from scratch (with women's intellectual history?) and leave aside the familiar, as this leads to "a nervewracking world, free-floating, with no structure, no reliable perspective at all" (p. 19). She will not abandon "conventions that have provided the basis of Western philosophy since the time of Plato and Aristotle," which are her "solid historical ground" (p. 19). Here, Ring should consider Luce Irigaray's "The 'Mechanics' of Fluids" in (1985) *This Sex Which Is Not One* and the "*complicity of long standing between rationality and a mechanics of solids*" (p. 107).

Certainly, such a solid book will not appeal to postmodernists even though it does tend to conflate feminist theories of difference with postmodernism. Nor is it a text that introduces and analyzes the engagement of contemporary feminism with modern political thought. Rather, it objectifies feminist—especially radical feminist—work. Modern political thought is masculine only: Hegel, Mill (with no mention of Harriet Taylor), and Marx. *Modern Political Theory and Contemporary Feminism* does not achieve the creation of a dialectical feminist epistemology. This is a project that will require attention to the substance and structure of women's experiences and writings and feminist thought.

University of Victoria, Canada SOMER BRODRIBB

Social Change, Social Welfare, and Social Science. By Peter Taylor-Gooby. Toronto: University of Toronto Press, 1991. 244p. $45.00 cloth, $18.95 paper.

What would be the most devastating criticism of the welfare state? It would consist of an explanation and an apology: "We're truly sorry; but as much as we would *like* to be able to address the needs of the poor, our economy cannot do so without destroying itself." Such a position is impeccably effective because it makes any normative exhortation (the heavy artillery of the liberal Left) simply irrelevant. This strategy has propelled the New Right to electoral victory in a number of English-speaking states for over a decade.

The most compelling defence of the welfare state would thus be to expose this claim as one of false necessity. The best defence would be more an illuminating account of economic causality than one of moral suasion. The most striking success of Taylor-Gooby's latest book is that it challenges the perceptions of what the constraints facing the welfare state both now and in the next century really are.

Taylor-Gooby enumerates the sins held against the welfare state and proceeds systematically to show that there is good reason to doubt the supposed inefficiencies of a comprehensive system of social welfare. How can we blame welfare policies for low economic growth "when high spending nations have as high economic growth rates in the 1980s as low spenders?" (p. 55). Why is there "no convincing evidence that the welfare state pre-empts resources and crowds out productive industry" or that high rates of taxation undermine the incentive to work? (p. 58).

Even the spectre of alarming shifts in demographic patterns is challenged. While it may be that the cohort of over-65s will increase significantly in the near future, it does not necessarily follow that a worrisome reduction in the ratio of wage earners to economically dependent individuals will result. By restructuring social policy to entice more women into the workforce, by revising immigration restrictions, by raising the age of retirement, and by ceasing automatically to equate "retired" with "a drain on resources," the welfare state would not be placing the productive capacity of the economy in jeopardy.

But what of *deliberate* strategic attacks by the Right? Is it possible for a government to engineer the collapse of state welfare by alienating middle-class support for decommodified social services? Citing the results of recent British Social Attitude Surveys, Taylor-Gooby argues, "There is little evidence to support the view that prosperous people wish to forsake the state sector. Their attitudes have grown closer to those of the less fortunate" (p. 126). It is useful to have this data at hand; and the more earnest defenders of the welfare state will be relieved to learn of the spirited support of the middle classes. But this information does raise enticing questions that are not fully addressed: If there is such strong public support for welfare policies, then why does the British public continue vigorously to return to office the very party that has attempted so valiantly to dismantle the welfare state? Is it significant for theories of the welfare state that the collective attitudes of, say, Californians differ so dramatically from those of the British? And is health care, which elicits the most passionate public support, representative of public attitudes toward welfare policies, or is it sui generis?

Perhaps the weakest section of an otherwise commendable book is the discussion of normative justification for welfare. Needs are essential in any consideration of moral life and, as such, require "access to services which will advance survival and good health to ensure equal respect for the 'plans of life' advanced by different people" (p. 187). This is not necessarily incorrect; but the attempt to establish basic human needs as "rights" in a single, short chapter results in a sketchy account that simply will not convince the unconverted. The effort to situate philosophical discussions of moral obligation cheek-by-jowl with empirical accounts of causality is a laudable venture; but in this study, it simply does not accomplish very much.

Social Change, Social Welfare, and Social Science is a superb overview of developments in welfare state theory, and it is a resonant challenge to those who proclaim that the welfare state is no longer economically viable. The book also incorporates a rich discussion of the influence of various policies on *women as a social class;* and since women are frequently those who have the most to gain or to lose from shifts in the welfare state, this perspective deeply enhances an already remarkable survey of welfare state literature. Ultimately, Taylor-Gooby succeeds most admirably in his defence of the welfare state through his modest reminder that we must judge redistributive policies not by measuring the gap between wealthiest and poorest but by noting the absolute gains that the welfare state has won in making many people's lives at least humanly bearable. If not a majestic accomplishment, it at least would mean a great deal to a very large number of people.

Dalhousie University K. FIERLBECK

AMERICAN POLITICS

Political Attitudes over the Life Span: The Bennington Women after Fifty Years. By Duane F. Alwin, Ronald Cohen, and Theodore M. Newcomb Madison: University of Wisconsin Press, 1991. 422p. $40.00 cloth, $21.95 paper.

The women of Bennington College in the 1930s have a firm place in social psychological folklore and textbooks. What many social scientists do not know, however, is that Newcomb's classic 1943 book, *Personality and Social Change,* was not a one-shot demonstration that impressionable women in late adolescence often shifted their political views to the left in response to social and intellectual pressure from the liberal professoriate (to the annoyance and chagrin of their upper-middle-class, largely Republican parents, who were paying hefty tuition bills in the midst of the Great Depression). That is only one slice (a misleadingly narrow one) of the whole story. The Bennington study represents one of the most systematic, nuanced, and sophisticated efforts to chart patterns of attitude change and stability through the life span.

Longitudinal studies of this sort require great patience. The investigator must confront the grim likelihood that he or she may not live to oversee the last wave of data collection. Newcomb passed away in 1983; but he was fortunate to have two thoughtful and conscientious collaborators—Duane Alwin and Ronald Cohen—to whom he could pass the longitudinal research baton. The result is a first-rate product that tells serious students of political attitudes virtually everything they need to know about the Bennington study.

The authors lay out three objectives for themselves: to describe the voluminous results of the three-wave longitudinal study of persistence and change in the political attitudes of several cohorts of Bennington women from the 1930s to the present, to interpret and explain these data patterns, and to explore more general conceptual and methodological issues that arise in studying the lifelong development of attitudes. Although there is plenty of room to quibble over detail and even room for raucous argument over certain big issues, the authors succeed on all three counts. There are, moreover, a few surprises.

The Bennington study is usually thought of supporting "critical stage" models of political socialization, which allow for the possibility that early family influence (the "primacy principle") can be trumped by later experiences if those experiences occur at the right time (e.g., late adolescence) and in the right form (e.g., articulate and impressive role models). This interpretation is not out-and-out wrong, but it is misleading. As the authors document, family of origin emerges as "one of the most important" determinants of political attitudes. The authors refrain, however, from causal overinterpretation. They wisely recognize the limits on the inferences they can draw about the relative importance of causal variables given the unique circumstances of Bennington in the 1930s and the confounding of historical period and human lives.

The results are also troublesome for the theoretical positions that attribute great potential for adaptation and change in political views in middle age and beyond. In contrast to this view, the authors argue that political attitudes are highly persistent from early adulthood to old age, especially when the women marry men with similar political orientations. Overall, the authors estimate that for the nearly 50-year period covered by the data, about 60% of the variance in the latent attitude variables could be predicted from 1930s attitudes, whereas 40% of that variation reflected attitude change. (The psychometric and conceptual problems in distinguishing true attitude change from mere measurement error should keep at least one generation of methodologists busy.)

The authors are also to be commended for drawing skillfully on auxiliary data sources (such as the National Election Studies) to answer otherwise unanswerable questions. One such example is the issue of whether the leftward shift in political attitudes among women in the Newcomb sample was the result of their experiences at Bennington or the product of a more general generational shift (a new Deal effect in which most people coming of political age at that time moved toward a more liberal worldview). The authors persuade me that there is still a Bennington effect, even after taking into account the New Deal effect.

To be sure, there are many problems that the authors cannot solve. Impressive though the span of time covered by the Bennington assessments is, we lack direct evidence on the early childhoods of the women. Data from Block's longitudinal project indicates, for example, that we can predict the liberalism–conservatism of young adults from observer Q-sort assessments of the same people on variables such as *ego resilience* versus *rigidity* and *overcontrolled* versus *undercontrolled.* This possibility raises, in turn, another possibility: susceptibility to change in later life may be the product, in part, of deep underlying dispositions acquired in early life (dispositions that Eysenck and Wilson argue may even be grounded in genetic causes).

Limitations aside, however, this book stands a fitting capstone to a pioneering research project and a fitting tribute to the career of a major social psychologist.

University of California, Berkeley PHILIP E. TETLOCK

The Vital South: How Presidents Are Elected. By Earl Black and Merle Black. Cambridge: Harvard University Press, 1992. 400p. $29.95.

Interest Group Politics in the Southern States. Edited by Ronald J. Hrebenar and Clive S. Thomas. Tuscaloosa: University of Alabama Press, 1992. 432p. $27.95 paper.

A number of years ago, a line in a popular country-and-western song urged listeners to "save your Confederate money boys, 'cause the South's gonna rise again!" In the years since this song enjoyed regional popularity, the South has indeed risen. The region has enjoyed unprecedented economic growth accompanied by population increases sufficient to make the South the nation's most heavily populated region. Earl and Merle

Black demonstrate, in *The Vital South* their second volume on southern politics, the central role of the region in the nomination of presidential candidates.

In meticulous detail, the authors trace the shift in southern loyalties from the era of the solidly Democratic South to the centrality of the South as part of what has been described as the Republican presidential lock. Following the early chapters examining regional voting patterns from the time of the Civil War, the bulk of the analysis concentrates on the recent past. Using data from the National Election Studies, as well as exit polls conducted by CBS News–*New York Times*, the electorate is classified into white, core Democrats; white, core Republicans; swing whites and blacks. The outcomes of recent presidential elections in the South can be explained largely in terms of the reaction of the swing, white voters to the options presented. In accounting for Republican presidential victories in the South from 1972 through 1988 (excepting 1976), the authors return to a theme of their earlier *Politics and Society in the South* (1987), namely, that the successful Democratic politicians are those who can blend liberal and conservative themes into a harmonious symphony, while those who insist on a single liberal melody, like recent Democratic presidential nominees from the North, are condemned to defeat.

While exploring recent southern preferences in the presidential arena is the authors' chief concern, the South is not examined in isolation. In demonstrating that the South, which was the foundation stone for the Democratic party for generations, has become the keystone in a triumphful GOP arch, the authors emphasize what a party must do to win an electoral college majority, given a specific level of performance in the 11-state South. A cohesive South, according to the authors, holds the key to victory (and the South has usually voted as a bloc in presidential elections). A candidate who sweeps the South, whether it be Franklin Roosevelt or George Bush, can win the electoral college with relatively few states from the rest of the country. As the South becomes home to a growing share of the American public, its potential in presidential elections, when united, increases. As the South goes, so goes the nation.

Southern influence is not limited to the general election. The authors trace the role of the South in the nomination process in the years since the choice of delegates to national party conventions through primaries became widespread. On the Democratic side, Carter's trouncing of Wallace in Florida in 1976 and Mondale's victories in Alabama and Georgia went far toward sealing the outcome of those nomination battles. In like fashion, the refusal of the South to defect to Ronald Reagan in 1968 ensured Richard Nixon's nomination, just as did solid southern support for Reagan in 1980 and George Bush in 1988 put down the efforts of their rivals. Beginning in 1972, the South has been instrumental in the nomination and election of the next president.

Political scientists have concentrated on explaining individual vote preferences. While the authors provide some micro analyses, the name of the game is winning states; and they convincingly demonstrate that so long as the South votes as a bloc, it can provide its favorite with a substantial lead in the race for the White House. For three generations after the Civil War, the South was so out of step with the nation that its preferences were rarely elected. For the next three generations, southern preferences have been sufficiently mainstream that rarely has the southern favorite among the nominees been defeated. Consequently, while a united South provides its choice with approximately half of the needed electoral votes, southern preferences are largely reflected in nationwide preferences, so that the South does not find itself in the position of holding the balance of power.

While the authors observe that "the emergence of a Solid Republican South has been a pivotal event in modern presidential politics" (p. 325), they do not foreclose the situation of 1992, in which Southern enthusiasm for the Republican nominee wanes. The authors warn that an economic collapse or the ineptitude of a Republican president can usher in Democratic success. An interesting question is whether, when given an opportunity such as was presented by Bill Clinton, the Democratic party can reverse the region's Republican leanings and create a biracial coalition and make the South safe for Democratic nominees.

The Vital South should not be viewed as just another attempt to update V. O. Key. By placing the progress of presidential campaigns in the South in the broader context of national politics, *The Vital South* will appeal to a broader readership than did *Politics and Society in the South* (1987). The authors' earlier book may have been overlooked by those having no particular interest in the nation's most distinctive region. Academics, practitioners, and interested citizens trying to understand presidential politics ignore the analysis in *The Vital South* at their peril.

Where Earl and Merle Black examine the mass behavior of the electorate, Hrebenar and Thomas present essays on the activities of interest group elites in 12 southern states (including Kentucky). This volume is the latest in a series, which, when complete, will provide case studies of the interest group constellation in all 50 states.

The 12 articles contain rich descriptive materials marshaled by 20 authors who know in detail the politics of the states in which they teach. By recruiting local experts to describe the interaction of groups in dealings with the three branches of government and in the electoral arena, this effort goes beyond earlier works by Harmon Ziegler, Hendrick Van Dalen, and Sarah Morehouse. Of course, allowing an entire chapter for each state provides far more room for development than in the works just cited, in which valiant efforts were made to encompass state-level group activity within a single chapter.

The editors exercise a loose rein over their 20 charges. A collection of eight major factors that may influence group composition and activity are set forth in the introductory chapter. These elements are of variable utility to the authors of the substantive chapters. For example, elements such as the professionalization of the state legislature, the strength of the executive and of political parties, economic development, and regulation of lobbying and campaign finance are touched upon in each state discussion. On the other hand, intergovernmental spending and policymaking authority is largely irrelevant, since the focus is almost exclusively on state decision making. While a few contributors reference the slippery concept of political culture, it is more ignored than utilized.

Each state chapter includes a brief political history as a context in which to place group activity over the last 20 years. Authors rely on filings with the appropriate state office to sketch a picture of the numbers and types of interests represented among registered lobbyists. Since state registration requirements vary, some inconsistencies are unavoidable. A number of states do not require legislative liaisons for government agencies to register as lobbyists; and a few states (e.g., Texas) have registration requirements so imprecise that the number of registered agents has declined sharply in recent years. Chapters generally report on the range of lobbying techniques used and often provide assessments of the relative usefulness of various approaches. Identifying the most important groups within the state is another feature of most chapters.

Despite similarities across chapters, substantial variation remains. Most, but not all, authors make use of a survey. Some follow the lead of Harmon Ziegler and Michael Baer by tapping perspectives of both lobbyists and legislators on issues of group and technique effectiveness. From a survey of lobbyists and legislators, Arthur English and John Carroll develop an index of change in the perceived influence of representatives of 24 players in Arkansas. In writing about Texas interest groups, Keith Hamm and Charles Wiggins draw a 15% sample of the bills introduced in the 1983 session to determine the number and types of groups involved on each bill, the degree of conflict over particular pieces of legislation, and conditions associated with success. Several other chapters (e.g., that on Mississippi) present perceptual information to show that lobbyists and legislators, while looking at the same events, frequently see them differently. The attention devoted to political action committees and their role in campaign finance varies from state to state, with one of the more extensive treatments given in Georgia.

As with the Hrebenar and Thomas volumes for other regions, their book on the South provides a wealth of information not otherwise available. The factors identified in the first chapter could be used to develop variables with which to test possible explanations for the variety, influence, or development of groups at the state level; but no such effort is made. The cross-state comparisons offered in the last chapter are disappointing. These efforts involve identification of types of interests actively represented in all southern states, as opposed to those found in 11 or fewer; comparison of the most effective interests today to those noted in Morehouse's work of a decade ago; and a seemingly largely subjective ranking of groups by effectiveness within the South. This South-wide ranking is not contrasted with that of groups of interest in the other three regions.

University of Georgia CHARLES S. BULLOCK III

Governance of the American Economy. Edited by John L. Campbell, Jr., Rogers Hollingsworth, and Leon N. Lindberg. New York: Cambridge University Press, 1991. 462p. $65.00 cloth, $21.95 paper.

All academics should post above their desks the eminent historian Lawrence Stone's credo, "One should always try to write plain English, avoid jargon and obfuscation, and [make] one's meaning as clear as possible to the reader." Unfortunately, the editors of this volume violate the credo, thereby preventing an interesting collection of papers from reaching as wide an audience as it should. An example is the following unfortunate description of the efforts of land grant colleges and other institutions to guide the direction of agricultural research: "Promotional networks typically bring together diverse actors from different parts of the production or service delivery chain for the purpose of the common promotion of a product, including the organization of research and development, the diffusion of information and lobbying the state" (p. 25). I do not want to belabor the point, but it is an important one when the gap between academic social scientists and intelligent laypersons continues to widen.

Part 1 consists of two chapters that purport to state and elaborate the theoretical orientation. Part 2 consists of eight industrial sector studies that attempt to apply the theoretical orientation. There is no pattern to the sectors, they are simply the contributors' choices: telecommunications, nuclear energy, railroads, steel, automobiles, dairy, meatpacking, and hospitals. Part 3 consists of two chapters in which the editors evaluate the theoretical significance of the case studies. Campbell alone among the three editors also contributed a case study (nuclear energy).

In order to grasp the book's framework, we must begin with Ronald Coase's 1937 article, "The Nature of the Firm," in which the concept of marginal cost was used to explain when firms would conduct transactions through market mechanisms and when they would attain their goals administratively. Coase's brilliant piece spawned a veritable avalanche of studies on the theory of the firm, contracting, and other organizational matters. The field became increasingly sophisticated, enriched periodically by the publication of path-breaking studies such as Harold Williamson's *Economic Institutions of Capitalism,* in which transaction-cost economizing was used to explain a variety of organizational behaviors, such as vertical integration and innovativeness. But while many noneconomists admired the rigor and elegance of transaction-cost analysis, they were bothered by the reductionist view that all human behavior is either economic or random. Heroism in war, obedience to one's religious convictions at great cost, and a host of other not uncommon behaviors do not fall easily into the economizing category.

This volume attempts to salvage the virtues of transaction-cost analysis, while at the same time taking account of such collective action as "voting, social movements, and voluntary associations, altruism and self-sacrifice, and obedience to rules, laws and customs that are embedded in social relations" (p. 15). But while the criticism of economic reductionism is not inappropriate, the authors fall short of providing an alternative framework that can embrace self-interest, as well as these types of collective action. We are, instead, provided with an insightful but limited extension of modern transaction-cost analysis. The editors introduce the central notion of *governance* to explain behavior. While the concept is nowhere explicitly and clearly defined, it is intended to include power, as well as the ability to procure resources. Thus, a firm will seek not only to obtain resources, but also to control the terms of exchange strategically. The editors list six ideal types of governance mechanisms, each with its essential rules of exchange. For example, the market's basic rule of exchange is classical contracting, the hierarchy's is administrative command, and the association's is formally organized membership agreements. The state is in a privileged position, influencing governance in numerous ways.

The case studies were intended to help determine which governance mechanisms tend to emerge, succeed, and fail in each sector. Like any conceptual device, governance must be judged by its heuristic utility. This, of course, places a heavy burden on the case studies. In general, the studies tend to cover too much in a relatively few pages. In every case, however, the reader is treated to an interpretation of an industry's history with scarcely a concession to the fact that there have been divergent ones. More importantly, governance adds very little to the more thorough studies of each industry that rely on traditional ideas of industrial organization, interest group, and public law to explain competitive behavior and state action. The state has been deeply involved in six of the eight industrial sectors covered. It has also been involved in steel and automobiles, though with a lighter hand. Not surprisingly, then, state action has helped to shape and reshape industries. But all of this does not sustain the editors' claim that they have developed a set of conceptions that will generate novel insights about the business–government interface (p. 356).

Nevertheless, persons working in this area should carefully read the book. The volume's conceptual apparatus may be precisely what they need to generate insights about their research topic.

University of Houston ALAN STONE

Creative Campaigning: PACs and the Presidential Selection Process. By Anthony Corrado. Boulder: Westview, 1992. 286p. $29.95.

Inside Campaign Finance. Myths and Realities. By Frank J. Sorauf. New Haven: Yale University Press, 1992. 288p. $27.50.

Of all the issues of policy on which political science might influence public discourse, campaign finance would seem to be a prime candidate. Money in politics goes to the very heart of issues of democratic theory and practice; and in evaluating the effects of possible changes in the regulatory regime shaping campaign finance, the discipline can draw on its rich stock of knowledge about elections and policymaking. Yet even allowing for the indirection with which social science becomes usable knowledge, political science's contribution to the national debate over campaign finance has been modest indeed. Fed by the media and latter-day progressives such as Common Cause, the rhetoric of reform is dominated by political bugbears; and in the absence of reasoned argument about consequences, Congress's handling of the issue has become a thinly veiled fight for partisan advantage.

Political science must bear part of the blame for its ineffectuality. The vast sets of data produced by the Federal Election Commission have been grist for the discipline's empirical mills; but the questions that have guided the exploration of these data have, for the most part, been far removed from the real world of campaign finance or the specific issues of reform. Indeed, as the field has been colonized by economists from the public choice school, the study of campaign finance has even tilted away from traditional sorts of questions about elections and political organizations and toward highly stylized models of the microeconomics of political markets, which offer little insight into politics or policy.

Creative Campaigning and *Inside Campaign Finance,* in contrast, are examples of political science at its best. Although the books differ in purpose and scope, both are steeped in a rich empirical understanding of the law and actual practice of campaign finance. Both emphasize the fact that campaign finance is a dynamic process marked by the strategic adaptions of major players. Both will serve as sources of knowledge and guides through the maze of campaign finance.

An important lesson of history is that regulating political money is like squeezing a balloon. The subject of *Creative Campaigning* is one of the unanticipated consequences of the Federal Election Campaign Act (FECA) of 1974—the rise of precandidacy political action committees by presidential aspirants. From Ronald Reagan's Citizens for the Republic, to Walter Mondale's Committee for the Future of America, to George Bush's Fund for America's Future, presidential candidates have created personal political action committees in order to exploit a loophole in the law that allows them to build large war chests and substantial organizations without the same limits on contributions and requirements of disclosure that regulate the finances of campaigns. In the 1988 election cycle, for example, the receipts of candidate-sponsored political action committees amounted to $25 million, most of it spent on fund raising and organization building, not on contributions to congressional candidates. Corrado, who has the double perspective of a political scientist and a staffer in several Democratic presidential campaigns, lucidly shows how the law and subsequent regulatory decisions of the Federal Election Commission produced this form of strategic and organizational adaptation by candidates.

Corrado argues that these "shadow campaigns" have had important effects on the race for the White House and that they pose problems for the system of campaign finance, which he proposes to deal with by improving disclosure and by loosening regulations on contributions to campaigns and state-by-state spending limits in the primaries. All of this makes sense, even if the author overstates the significance of these political action committees. After all, their total spending (a large share of which was consumed by costs of fund raising) amounted to less than 10% of the nearly half-billion dollars spent on campaigns for presidential nomination in the 1980s. And in the first campaign of the 1990s, these political action committees were scarcely a factor. Corrado suggests that 1992 was an anomaly and that precandidacy political action committees are likely to reappear in 1996. Regardless of whether Corrado is prescient, *Creative Campaigning* is important reading as a case study of the complexity of political regulation.

The goals and effects of regulation are also an important theme of *Inside Campaign Finance,* a sweeping and masterful account of the role of money in American elections. The book is vintage Sorauf—splendidly written, rooted in data and history, connected to important questions about politics and governance, and sober in its discussion of reform. The book begins with the best short history of campaign finance that I have read, as well as the best mapping of the contours of money in contemporary American politics. Having made the arcana of campaign finance accessible to his readers, Sorauf proceeds to help us fathom the logic of money and politics.

Exchanges in the political market are far more complex than reformers assume. As the rhetoric of "buying" politicians suggests, popular and scholarly wisdom holds that politicians are passive commodities in the play of interest groups. Yet, Sorauf argues convincingly, the transactions in campaign finance are not so one-sided. In fact, members of Congress have been remarkably adaptive in the post–FECA 1974 world, so much so that, in most cases, powerful incumbents in safe seats define the terms of political exchange. The FECA was also supposed to empower small individual contributors from the grassroots, but it begat organization and counterorganization instead. Bellwether political action committees and others, congressional campaign committees, brokers, bundlers—the rich variety of post–FECA organizational forms is testimony to the impulse to aggregate interests for political advantage.

If the market of political money is more complex than is commonly assumed, its effects are not as pernicious as is commonly feared. Sorauf examines the evidence about the most serious counts in the indictment of American campaign finance, including (1) that campaign money buys excessive influence, (2) that campaign money buys elections, and (3) that campaigns are too expensive. In each case, he finds the charge not to be supported by scholarly evidence. Yet these concerns, which are the legacy of populism and progressivism, dominate the media and public opinion and drive the debate about campaign reform. The result is a penchant for regulatory regimes that in theory squeeze money out of politics but in practice disappoint and breed even greater cynicism.

Political science has two important roles to play in shaping public discourse about money, elections, and governance. The first is to sharpen our understanding of how the system of campaign finance actually works. The second is to elucidate the principles by which we might judge the current system and alternatives and to point out how those principles may collide. *Creative Campaigning* and *Inside Campaign Finance* succeed admirably at both tasks.

Hamilton College THEODORE J. EISMEIER

The Administrative Presidency Revisited: Public Lands, the BLM, and the Reagan Revolution. By Robert F. Durant. Albany: State University of New York Press. 1992. 401p. $17.95 paper.

Reluctant Partners: Implementing Federal Policy. By Robert P. Stoker. Pittsburgh: University of Pittsburgh Press, 1992. 213p. $39.95.

The "reluctant partners" of Robert P. Stoker's title are federal administrative agencies and other participants in the implementation of federal domestic programs, such as state and local governments, community organizations, and business enterprises. Stoker's premise is that these partners often have serious conflicts and are unable to collaborate effectively. His book is an effort to understand how and why implementation succeeds or fails.

Primarily a theoretical essay, the book develops a "regime perspective" for the analysis of policy implementation. Contrasting this perspective with conventional approaches that stress authority or exchange, Stoker borrows from work by Robert Axelrod, Robert Keohane, Kenneth Oye, and Oran Young on regimes and cooperation in international relations, and from the work of Clarence Stone on urban regimes. He argues that successful implementation requires "inducing cooperation within a context of diffuse authority" (p. 14). His approach aims to identify the "contextual conditions, organizational strategies, and policy actions" that help produce such cooperation (p. 18).

In the central theoretical chapter, Stoker starts with a lucid presentation of some game-theoretic analysis of problems of cooperation. He demonstrates the barriers to cooperation in the Prisoner's Dilemma, the even greater barriers in Stalemate, and the more manageable yet still dangerous situations in Stag Hunt and Chicken. Following the international relations theorists, he cites several general factors that can overcome the difficulties, such as linkages among issues, enhanced rewards of cooperation, established rules of conduct, expectations of constructive interaction in an ongoing relationship, and division of a program into a series of small exchanges. Stoker suggests that making use of such factors is the essence of designing an effective implementation regime.

There are some limitations in Stoker's development of these promising notions. Apart from discussing a handful of examples, he does not address, in a general way, either what kinds of programs, administrative structures, or other circumstances lead to the different kinds of cooperation problem or what kinds of measures are available to create linkages, enhance rewards, establish expectations, and so on. Instead, the succeeding chapter presents a typology of implementation regimes based on the degree of governmental or private responsibility and the concentration or diffusion of governmental authority. Although this typology leads to some interesting observations, it represents an entirely different concept of a regime and does not carry forward the theoretical perspective.

Nevertheless, two case study chapters provide some support for the regime perspective. An account of the development of the school lunch program shows that implementation can be highly responsive to changing federal priorities if an effective regime, with expectations of cooperative action, has been established. A second case study, which compares federal siting policies for high-level and low-level nuclear wastes, demonstrates the effectiveness of carefully designed inducements for cooperation.

As Stoker points out, the regime perspective has some provocative policy implications. In contrast with conventional wisdom, it argues that long-established implementation regimes are likely to be more effective than new ones, even for serving new purposes. And it suggests a strategy of initiating programs with limited demands and a large emphasis on distributive benefits to foster cooperative relationships that will support heavier demands at a later time.

Stoker's fundamental claims are persuasive and very important. Theories of regimes and cooperation should become prominent in research on implementation. Although its potential contribution is not fully realized, his perceptive and innovative book should help set the agenda for subsequent research.

Addressing more specific issues of presidential strategy, Robert F. Durant's book is one of the most substantial and penetrating empirical investigations yet made into the consequences of an aggressive administrative strategy for presidential leadership. Based on extensive research on Bureau of Land Management (BLM) activities in New Mexico during the Carter and Reagan administrations, it undermines any easy assumption that Reagan's pronounced centralization and politicization of the bureaucracy actually served the president's interests.

To begin with, Durant criticizes research that evaluates leadership methods on the basis of immediate administrative objectives, such as budget cuts, reorganizations, or high-level policy statements. He points out that these objectives are largely instrumental and insists on looking beyond them to actions in the field and policy outcomes—whether production of oil and coal on public lands was increased, whether land was transferred to private use, and the like. He avoids supposing that presidents know their own interests with respect to administrative methods (as Terry Moe's seminal article stipulates they do).

Durant's selection of case material is appropriate in several respects. The BLM was important to Reagan's policy agenda (which sought to respond to the "Sagebrush Rebellion"); he employed strategies of politicization and centralization in the BLM and the Department of the Interior with exceptional vigor; and New Mexico was a central arena for the administration's effort, with singularly extensive public lands. On the basis of exceedingly thorough research, Durant recounts BLM administrative developments and policy performance in six areas: rangeland management, energy leasing, water resources management, wilderness preservation, urban economic development, and public land acquisition.

The findings of his well-crafted case studies are clear-cut. The Reagan administration had very limited success in altering BLM policy outcomes in New Mexico. More important, Reagan's administrative strategies contributed materially to its difficulties: budget cuts deprived the BLM of resources needed to effect policy change; appointees selected directly from one affected industry (ranching) blocked actions intended to benefit a rival industry (minerals extraction); and ideological zealots ran afoul of legal and political constraints, embroiling the agency in unproductive conflict. Durant's research suggests that presidents have compelling reasons to emphasize professionalism and neutral competence as important elements of their administrative strategy.

I have a few complaints, which may not be generally shared. First, Durant strives for an artful, literary prose but ends up overwriting, with a result that is often tedious or distracting. Second, and more important, the book's theoretical commentary is unwieldy. The final two chapters contain almost one hundred pages of theorizing that goes well beyond the book's research issues of presidential strategy to address the most general questions of organization and management. Drawing on analogies from evolutionary theory and from Robert Golembiewski's recent work on organizational structure, among other sources, this work appears to be quite original. It did not strike me as highly persuasive (although it clearly impressed the reputable author of a promotional blurb that appears on the cover).

Whatever the discipline comes to think of Durant's broader theoretical effort, his research and analytic arguments about presidential management represent a major contribution.

University of Illinois,
Urbana-Champaign

PAUL J. QUIRK

Political Protest and Cultural Revolution: Nonviolent Direct Action in the 1970s and 1980s. By Barbara Epstein. Berkeley: University of California Press, 1991. 327p. $24.95.

The photograph of the author on the typical academic book jacket is perfectly predictable: there she is, at her desk in her campus office, surrounded by books. Barbara Epstein's photograph is different. It shows her, as the caption says, "under arrest, being escorted to a police van, San Francisco, 1990." *Political Protest and Cultural Revolution* is her account of the nonviolent direct action movement, told from the perspective of one very much involved in it. As a participant, Epstein sympathizes with the movement's goals and wishes it success. As a scholar, however, she finds herself highlighting the movement's failures, as well as its successes; and the tension, captured in the jacket photograph, of the author as scholar and activist makes the book unusually interesting even though, in the end, it is not a complete success.

The direct action movement Epstein describes is essentially made up of people "who believe in nonviolence, engage in political action through affinity groups, practice decision making by consensus, and employ the tactic of mass civil disobedience" (p. 1). Their politics is utopian, aiming at cultural revolution and the transformation of social relations as much as political and economic change. The book focuses on three of their actions: the Clamshell Alliance's attempt to halt construction of a nuclear power plant at Seabrook, New Hampshire, in the mid-1970s; the Abalone Alliance's attempt to prevent the opening of the Diablo Canyon nuclear plant in California in 1981; and the Livermore Action Group's effort to shut down the Lawrence Livermore National Laboratory shortly afterward. Epstein's main purpose is to determine whether the direct action movement "can sustain itself over time and be effective in helping to change society" and to consider the implications of its successes and failures for political change in the United States (p. 16).

Drawing on movement documents, interviews, and (most importantly) her experiences as participant, Epstein finds some evidence of movement success. She argues that the Abalone Alliance's occupation of the Diablo Canyon plant area indirectly led to a long shutdown and that although the Clamshell Alliance failed to halt construction at Seabrook, it played a role in generating opposition to nuclear power and created a basis for future action by providing thousands of participants with training in nonviolent direct action and with intense, meaningful political experiences.

Movement failures are at least as significant as movement successes, however. No targeted facility was shut down directly by nonviolent direct action. More importantly, Epstein argues, movement ideology makes it difficult to learn from past mistakes and improve effectiveness. The dominant ideology opposes hierarchy and formal leadership, requires that decisions be arrived at by consensus, is ambivalent (at best) about attempts to win media attention, and opposes systematic thinking about long-term strategies. As a result, movement organizations have no satisfactory way to deal with internal conflict, recruit and replace leaders, develop expertise, or expand their base of support. They are created for specific purposes and disband when participants either become alienated or feel they have accomplished something, so each campaign involves recreating an entire organization.

In addition, the ideology affects recruitment and the possibility of influencing American political discourse. For many participants, involvement in direct action is part of a countercultural life-style merging the personal and political. This often makes participation emotionally satisfying but limits the movement's appeal to the wider society, as the paganism, anarchism, and lesbianism of many participants is broadcast along with messages about narrower political goals (such as stopping nuclear power). And participants' disinclination to intellectualize prevents their accomplishing what Epstein sees as potentially their most important task—to develop a "hegemonic project" with which to oppose the political Right by providing Americans a comprehensive alternative vision of a future society (p. 259). Only such a vision, Epstein suggests, can enable those who want change to compete with the Right and bring about transformative political change.

Epstein does not provide such a vision. It is difficult to fault her for not doing so, because her failure simply reflects the weakness of radical politics that she describes so well. But it is possible to fault her for taking too narrow a view from a political science perspective. Epstein is especially concerned about both the power of movements to transform consciousness and the organizational, as well as ideological, resources needed to do so. Her treatment of these subjects would have been enhanced had she paid more attention to recent work on social movements (by William Gamson, David Snow, and others) and democratic politics (by Robert Dahl and others). Epstein assumes that we know less than we do and so concludes the book with less useful, and more pessimistic, conclusions than she otherwise might. But placed in a broader context of literatures on social movements and democratic politics, this is a useful, well-written, and thoughtful book.

University of Washington, Seattle PAUL BURSTEIN

Leadership in Committee: A Comparative Analysis of Leadership Behavior in the U.S. Senate. By C. Lawrence Evans. Ann Arbor: University of Michigan Press, 1991. 210p. $32.50.

The Emergence of a Senate Leader: Pete Domenici and the Reagan Budget. By Richard F. Fenno, Jr., Washington: Congressional Quarterly 1991. 250p. $29.95 cloth, $19.95 paper.

Scholars long have debated the relative weights we should place on personality and context in understanding political (and other) leadership. In *Leadership in Committee* and *The Emergence of a Senate Leader*, Lawrence Evans and Richard Fenno argue for context. To be sure, character, experience, and individual goals are not dismissed. But both scholars argue that contextual factors (most importantly, the preference architectures that surround them) place leaders within constraints that limit their ability to achieve personal goals.

Based upon extensive interviews and personal observations, both studies offer students of Congress a richly textured, inside view of politics in the Senate. In each case, we learn firsthand of the constraints leaders face, the frustration that comes with position, and the importance of parliamentary sequence to the operation of the Senate.

Evans's goal is to advance our knowledge about leadership through the contemporary, comparative analysis of eight Senate committee leaders. His foundational assumption is that leaders behave differently. This variation can be observed in four key areas: the use of agenda prerogatives, interactions with subcommittee leaders, interactions between the two full committee leaders, and the extent to which leaders engage in anticipated response. Explanations for variation in these dependent variables are sought from two contextual variables (preferences and structure) and three individual characteristics (policy preferences, experience, and proximate career plans). From the outset, Evans makes it clear that context rather than character, is most helpful in explaining leadership activity.

Although Evans's analysis is not explicitly intended as an entry in the debate regarding committee power, the centrality of preference architecture to his study makes a contribution to that somewhat bloodless debate. Inevitably, committee leaders are faced with preference distributions within their committees and within their chambers that condition their success. These distributions are most crucial where agenda control and anticipated response come into play. In addition, committee structure (conceptualized here as centralization/decentralization) has an independent impact on leadership because it affects information resources, sequence, and the costs of leading.

The most obvious superficial difference in Fenno's study is that it focuses upon a single leader—Republican Budget Committee chair Pete Domenici—during the 1981–86 period of Republican rule in the Senate. More importantly, however, Fenno's analysis is not motivated by leadership. His central organizing concept is, instead, the political career. Domenici's leadership position is the animating characteristic of this particular subject, just as a presidential bid defined Fenno's examination of John Glenn, failed incumbency that of Mark Andrews, "adjustment" that of Arlen Specter, and reputation that of Dan Quayle. In isolation, this study is a splendid examination of leadership and its relation to a political career. But readers also need to see it as one piece in Fenno's emerging examination of political careers in the Senate.

In *The Emergence of a Senate Leader*, Fenno offers us a "framework for the study of a career segment" (p. xiii). Of the five Senate studies currently available, it is his most explicit presentation to date of the underlying model. At its core is what Fenno calls the controlling sequence: campaigning–governing–campaigning. Each sequence is marked by two transitional influences: (1) an electoral interpretation (in effect, a post hoc explanation of "what happened") is formed that shapes the turn or return to governing, and (2) an explanation of the governing experience shapes the subsequent campaign. Finally, each governing phase features four activities (adjustment, accomplishment, style, and reputation) that allow additional points of analysis.

Because Fenno's framework is career-oriented, it is necessarily more longitudinal. As a result, we see Domenici's leadership style emerge from his personal character and his personal goals—to become a "player" and to preserve the budget process. Evans's comparative framework is necessarily more cross-sectional; so, to some extent, we miss that developmental angle. But he more than makes up for that through direct comparison using the context, personal, and behavior variables. In each case, a picture emerges (consistent with most recent Senate scholarship) of constrained leadership. The Senate's flat structure and individualist orientation make leadership difficult. From Evans and Fenno we learn again that information is power, that procedural prerogatives lend influence to position, and that sequence and its creative use offer advantages.

Not surprisingly, both studies leave us with matters worthy of future research. First, without addressing it specifically, both studies suggest that the presence of dual leadership positions has some impact in the Senate. Senators inevitably hold more than one formal leadership position, a circumstance worthy of more systematic analysis for both career and leadership studies. Second, leadership is presumably conditioned by the character of legislation; that is, legislation embodying new ideas places leaders in a different strategic and tactical circumstance than incremental or routine reauthorizations do. Third, if committee leaders are constrained, then some examination of managerial qualities, as distinct from leadership qualities, might be useful. Fourth, in each case, committee structure is taken as a given. Presumably, however, the same type of forces that shape leadership are also at work (though perhaps in an antecedent fashion) on structure. The evolution of structure deserves continuing attention.

Finally, and for understandable reasons, Fenno's analysis suggests that the external political environment (the Reagan fiscal revolution) was particularly important in understanding Domenici's period as chair. In Evans, by contrast, the political environment outside the Senate receives less formal treatment while committee and chamber context receive much more. My guess is that these somewhat divergent emphases are partly due to subject and partly due to method. Regardless, it suggests that the connections between external and internal environments continue to be important and worthy of explicit treatment.

Evans and Fenno have produced rich additions to the study of leadership. Most importantly, they suggest distinct but complementary frameworks for doing so. More than just isolated examinations of individuals, each presents a set of analytic factors that will inform, encourage, and structure future scholarship. Leadership has been a perennial concern of political science and is disciplinary forbears. Political careers are a more modern focus for analysis. As these two important works demonstrate, there is much in common.

George Washington University CHRISTOPHER J. DEERING

Mayors and Money: Fiscal Policy in New York and Chicago. By Ester R. Fuchs. Chicago: University of Chicago Press, 1992. 361p. $42.00 cloth, $15.95 paper.

Ester Fuchs has given us an important and ambitious comparative study of fiscal policymaking in New York City and Chicago from the Great Depression through the 1980s. In particular, the author explores the causes of the emergence of a massive fiscal crisis in New York in the mid–1970s compared to Chicago's relative fiscal stability. For Fuchs, political institutions and history matter. Discounting explanations rooted in economic differences between the two cities, she finds these divergent fiscal outcomes to be rooted in the different political structures and processes that emerged in the two cities in the 1930s.

Fuchs argues that the rise of the powerful Cermak–Kelly–Nash Democratic machine in Chicago forced interest groups to moderate their budgetary demands and channel them through the party organization. The machine also aided the mayor in centralizing the fiscal policymaking process. Finally, the Democratic organization's influential state legislative delegation reduced municipal fiscal pressures by forcing state government and special districts, rather than the city, to assume responsibility for costly redistributional programs. Machine control, budgetary centralization, and a lean municipal service menu allowed Chicago to weather the recession of the 1970s despite signs of a weakening local economy.

For New York, however, the author suggests that the 1930s witnessed the weakening of machine fiscal discipline. Tammany Hall lost power to Fiorello La Guardia's long-lived reform coalition. Interest groups replaced the party organization as key players in local politics. Forced to construct ad hoc electoral alliances without a durable machine to discipline group demands, mayors won elections by spending. Reformers distrusted executive power, forcing mayors to share fiscal policymaking with other groups, particularly the Board of Estimate.

New York's unique brand of Depression Era liberalism also meant that the city, not state government or special districts, assumed responsibility for a wide range of costly social welfare programs. As a consequence, the city's mayors went on an uninterrupted 40-year spending spree, culminating in the de facto bankruptcy of 1975.

This book is much more than a tale of how politics and history matter to the relative fiscal stability of the two cities. The author also makes a normative argument about how the New York City fiscal crisis has been used nationally and locally to reorder fiscal priorities toward the business community and away from minorities and the poor.

There is much that is path-breaking in Fuchs's book. Methodologically, this is one of the few truly comparative–historical studies of urban fiscal policy. As such, the research transcends the limits to causal inference posed by the single-city case approach. For Fuchs, history cannot be reduced to time series analysis. Viewing Clio in all of her dynamic and ever-changing complexity, the author points out how epochal periods of regime change (in this case, the 1930s) reshape political institutions and incentive systems and thus structure subsequent patterns of policymaking.

While the author's theoretical argument about the fiscal policy differences flowing from party-based and interest-group-based systems is less original, her mastery of these two major urban cases and of the historical budgetary data makes this one of the most compelling accounts of the thesis available. The author breaks new methodological ground by exhaustively reconstructing from census materials a comparable series on city finances from 1929 onward. Her useful guide to census budgetary categories and historical data sources (app. C) should stimulate the launching of other historically based comparative urban policy research projects.

Yet this admirable book is not without weaknesses. The heart of the book, consisting of chapters 4 and 5, examines changing expenditure, revenue, and indebtedness patterns in New York from Jimmy Walker's administration to Ed Koch's and in Chicago from Anton Cermak's mayoralty to Eugene Sawyer's. Here, the author concentrates upon the complexities and richness of her dependent variable at the expense of explicating her independent variable. Fuchs has unearthed a rich budgetary data mine and is eager to show us all of her empirical nuggets. In the process, however, the overall argument that political institutions matter is somewhat lost; and the essentially political character of fiscal policymaking in the various mayoral administrations is muted.

Mayors and Money is an important addition to the literature on urban fiscal policymaking, municipal fiscal stress, and the New York City fiscal crisis. The writing style is direct and unadorned, the prose, spare and concise. Given its topic, approach, and data sources, the Fuchs book will have broad interdisciplinary appeal to historians, economists, and sociologists. This work should stimulate further exploration of such crucial questions as why the Great Depression produced different urban fiscal policy regimes and what difference these divergent regimes might make for the quality of life of the underlying population.

University of California, San Diego STEVEN P. ERIE

Legislative Party Campaign Committees in the American States. By Anthony Gierzynski. Lexington: University of Kentucky Press, 1991. 144p. $26.00.

We now know a great deal about the allocation decisions of the national party campaign committees. The works of Gary Jacobson and Paul Herrnson, drawing in part on expenditure data from the Federal Election Commission, have drawn a convincing portrait of organizations that seek to target their resources to closely contested elections but face constraints imposed by the demands of incumbents. The nonfinancial resources provided by these committees have been thoroughly described; and perhaps the new commission disclosure of soft money allocations will enable scholars to understand this behavior more completely, as well.

We know much less about the activity of party committees at the state level. Although some scholars (especially Ruth Jones) have done high-quality work on the financing of state elections, the allocations of state party committees in state legislative contests has not been systematically explored. Gierzynski's book is the first such effort. As such, it fills an important gap in our knowledge.

Gierzynski examines the allocation behavior of legislative caucus campaign committees (similar to the party congressional campaign committees at the national level) and leadership campaign committees (resembling national candidate political action committees). He has gathered data on California, Illinois, Indiana, Minnesota, New York, Oregon, Tennessee, Washington, and Wisconsin regarding committees of both parties in both legislative chambers. This data collection is an impressive enterprise and is supplemented by in-depth interviews with committee leaders.

Gierzynski tests a series of hypotheses with ordinary least squares regression. Depending on the percentage of candidates who receive no aid, it is possible that Tobit would have been a more appropriate technique. He hypothesizes that party committees and member political action committees will give more to close races and that those in states with a high probability of a change in majority party in the legislature will fund both nonincumbents and incumbents. He expects leadership political action committees to be less likely to fund nonincumbents. All of these hypotheses seem reasonable and are generally confirmed. To his surprise, leadership political action committees are quite willing to fund nonincumbents, although I suspect that this might seem less anomalous when the average party turnover during the tenure of party leaders is considered. Many state party leaders see seats turn over several times during their time in power and must cultivate newcomers, as well as incumbents. At the national level, party leaders help raise funds for challengers and open-seat candidates. Tony Coehlo's successful bid for the party whip was aided by the votes of several grateful junior members.

The book contains only 124 pages of text, which raises in my mind a question of the best way to present these results. Two or three journal articles could have communicated these findings, but a book format usually offers scholars the opportunity to be more expansive in their theory and also in their deep description. Gierzynski does not fully take advantage of these possibilities. I would have enjoyed a book that was half again as long, with perhaps a bit more theory and description. I can imagine a chapter describing the context in these nine states, including the degree of competition in state elections, the costs of elections, the party structures and histories, and the legal constraints. This would also have enabled me to determine what part of the picture the disclosure processes in these states hides. The national party allocation data do not show us the channeling of soft money to House and Senate candidates, and I suspect that there are similar problems with the disclosure at the state level. Gierzynski discusses the problem briefly; but without a more detailed description of campaign finance in these states, it is impossible for me to assess the adequacy of these data. I would also like to have seen more alternative model specifications, for I can imagine some interaction effects that appear not to have been tested. Perhaps the economics of publishing demanded a brief book, but I found myself wishing for more. However, the book is a relatively quick read, which also has its advantages.

I also wish the book had been better situated in the literature. The bibliography is only three-and-a-half pages long. There are only two references to the work of Ruth Jones, no mention of Ross Baker's *The New Fat Cats*, no mention of several conference papers in the last several years on state legislative political action committees, and several conference papers that had been published long before this book saw page proofs. The point is not to add gratuitous citations but, rather, that this work could have benefited from integrating some of this earlier literature.

Despite these caveats, this book is a useful addition to the literature. It is brief and focused and could be adopted as a supplement in graduate courses in parties, elections, or state politics. Gierzynski has done us a service by providing a start toward a systematic account of the allocation of money by state party committees. I hope he will continue these investigations and inspire others to follow a similar path.

Georgetown University CLYDE WILCOX

Reconcilable Differences? Congress, the Budget Process, and the Deficit. By John B. Gilmour. Berkeley: University of California Press, 1990. 257p. $35.00 cloth, $12.95 paper.

Since its beginning, scholars and political observers have labeled the new budget process a failure. John B. Gilmour argues, instead, that the budget process has been a success. It provides the tools Congress needs to exert its collective will over the budget and reduces the power of factions and committees to obstruct and delay legislation.

To Gilmour, legislators are "complex individuals with conflicting preferences," who are concerned about programs for their districts and with the national impact of budgets and deficits (p. 6). With these mixed motives, members of Congress will vote to change structures that they believe make them act irrationally, that is, deny acceptable trade-offs between these two interests. The budget process in the early postwar system was well designed for a bottom-up, piecemeal construction of the budget and poorly constructed for overall budgetary control. Still, the broadly supported ethos of responsible behavior that permeated the appropriations and revenue committees usually produced small budget deficits. But slower economic growth, tighter revenue, and increasing entitlements strained the old system from 1966 to 1972; and Congress moved to reform its "irrational" budget process. Gilmour argues persuasively that the 1974 budgetary reform was not designed to limit spending but, rather, to provide coordination in the budget process and restore Congress's institutional role in the budget process. He usefully terms the new budgeting process *iterative*: Congress considers budget totals and allocations simultaneously, rather than allowing one to subordinate the other.

The final three chapters analyze budget process mechanics under iterative budgeting. The first and most important analyzes the use of the reconcilation process to cut entitlements after 1980. Proposing that "Congress has become a majoritarian body," Gilmour demonstrates that in the new budget process, the most important decisions are made outside the committee structure by parties, caucuses, and bipartisan coalitions—or any group that can stitch together a budget agreeable to the majority of the body (p. 94). No specific entity has consistently directed the process. Gilmour convincingly details the declining significance of the budget committees after the introduction of reconciliation and the shifting sands of budgetary coalition formation in the 1980s. The penultimate chapter indicates how the reconciliation process forces cooperation from recalcitrant committees. The last chapter, on the Gramm–Rudman budget reforms, offers little that has not already been said elsewhere.

The book's most notable weakness is the omission of any political explanation of what has gone wrong in budgeting. If current budget problems derive not from the budget process but from the failure to achieve consensus on budgetary priorities, then it would be useful to spell out explicitly just what major developments have prevented this consensus from emerging. Certainly, part of such an analysis should be the changing evaluations of Keynesian management techniques, but the book ignores the fate of Keynesianism and its effect on budget politics. Are we seeing a systemic, or merely a cyclical, problem? Are we seeing a problem in budgeting or a more general crisis in American politics? One related question worth pursuing would be whether the majoritarian structure of the new budget process is so at odds with the pluralist structure of the constitutional system as to be permanently ineffective. Such a query raises provocative questions about Congress's ability to be a leading player in fiscal policy and the possibility that deficits are unintended consequences of reform.

The old budget process presents a second problem. To Gilmour, the old system was irrational because members could not pursue their twin interests in district programs and overall budgetary control. Yet the system worked well enough in both regards until the mid-1960s. Certainly, the system would be patently irrational if Congress had indicated, after World War II, that it was interested in being the driving force in fiscal policy. But Congress delegated that particular budgeting job to the president. Congress typically accepted the broad outline of the president's budget and then trimmed and adjusted as necessary to produce either a larger or smaller deficit. While this may not be an ideal way to conduct fiscal policy, it does provide some overall budgetary control and is not irrational, given Congress's stated willingness to let the president set the general parameters of budget policy. Reform would appear to be less the ascendancy of rationality over irrationality than a reassertion of institutional prerogatives.

Despite these weaknesses, this book provides a coherent account of recent changes in the budget process and properly warns us that political problems cannot be squeezed out by changing institutional structure. Methodologically the author relies on interviews and primary sources such as floor and committee debates, committee reports, and press accounts. For budget scholars, the material on reconciliation and the changing position of committees will be key; others with a more general interest in Congress will also find useful the analysis of the collapse of the old budget process and the rise of the new.

University of Wisconsin, Madison JOHN J. COLEMAN

Live from Capitol Hill: Studies of Congress and the Media. By Stephen Hess. Washington: Brookings Institution, 1991. 178p. $22.95 cloth, $8.95 paper.

Everyone with an interest in how the news media cover Congress is undoubtedly familiar with Stephen Hess's earlier books published in the 1980s. The volume at hand, the fourth in his "Newswork" series, exhibits some similarities with the previous books but is somewhat different, as well. Hess's usual methodology, if you will, is part content analysis, part interview, part questionnaire, and part observation. It is an approach that sometimes allows him to generate the kinds of data that can be used to test hypotheses. He is sensitive to issues of sampling without becoming so fussy as to pass up a juicy example or anecdote that might not have emerged in the formal data collection. Sometimes his efforts yield nothing more (or less) than intriguing insights and personal interpretations.

In any case, the reader learns quite a bit, usually in the form of confirmation or debunking of conventional wisdom. The writing is occasionally lively, usually clear, and infrequently a little ponderous where numbers are concerned.

This book is less thematically coherent than the prior volumes because it consists of a series of chapters ("studies") that could conceivably stand alone as scholarly or semischolarly articles. The titles are clever, verging on cute (e.g., "Watching the Watchdog," "Do Press Secretaries Change Light Bulbs?," "The Lowly Press Release"). One of the best of these pieces, "Live from Capitol Hill!," sheds light on how local television covers (or ignores) Washington generally and Congress and its members more specifically. Here, Hess demolishes the facile conclusion that incumbents have roundly succeeded in making themselves fixtures on hometown/home state television. Congress (especially the House) turns out not to be the cynosure of local television news attention.

In studies of how the congressional press corps regulates itself, how important press secretaries are to their bosses, what "lowly" and omnipresent press releases are used for, how senators and certain House members have used op-eds in prestige papers to shape policy debate, and how the demographics of the Washington press corps have changed since Hess's earlier study in *The Washington Reporters* (1981), he demonstrates repeatedly that while most members of Congress care about good press, the pursuit of it is neither as all-consuming nor as efficacious as some would have had us believe during the last decade. To be sure, press releases flow, videotapes get made, radio actualities get spewed out, and reporters slog around looking for sound bites and exclusives from the local incumbents (especially senators). But when the accounts are reckoned, there is not all that much news about Congress produced in either the electronic or press media.

By far the greatest virtue of this small book is that Hess evokes better than anyone else the ethos of media work on the Hill, from the perspectives of reporters, press staff, and members themselves. The author thinks of interesting ways to test ideas about what is genuinely important to media and political elites. For instance, in an effort to ascertain just how influential press secretaries are in an institution that is alleged to value good press above almost everything else, Hess looks at their salaries and determines whether there are relationships between their productivity, their self-assessments of importance, and their compensation relative to staff colleagues.

Hess's data analysis is typically bare-bones and descriptive, and his interpretations are sometimes cautiously idiosyncratic; but the message gets through. He does not push his data harder or further than they warrant. And his tone is one that invites us to explore further—just as he himself utilizes the work of many scholars who have written on Congress and the media.

Hess argues in the end that he has given us a book about the unimportance of the press to Congress, as compared with its centrality to presidential politics. He is right, and he has a good explanation for why more scholars and journalists have not recognized this fact. Politicians like to be seen (and heard and read and read about). Television is particularly seductive to many of them, who seek air time whenever possible. Hess writes:

> Getting on the air is an advantage, even if an exaggerated one. The costs are small, both in time and money, and the money is provided by taxpayers or campaign contributors anyway. Also, because most legislators sincerely wish to be noticed, there is no longer a stigma—the "show horse" label—attached to those who are exceedingly good at getting themselves on television. (pp. 108–9)

Members enjoy "celebrity." They seek it, and they dwell on its occasions. They may indeed deceive themselves that they are being more closely scrutinized by press and voters than, in fact, they are. Scholars may thus mistake the legislator's grasp at television celebrity or exultation at the publication of a *New York Times* op-ed as valid indexes of the political consequences of successful media utilization. In the end, these may be little more than bits of political self-indulgence, little noted and soon forgotten by those to whom it was directed—a sobering thought for a nervous incumbent with a nose for the camera. Stephen Hess may be slightly overstating his case, but it will be up to others to tell us why.

Union College CHARLES M. TIDMARCH

Rethinking Social Policy: Race, Poverty, and the Underclass. By Christopher Jencks. Cambridge: Harvard University Press, 1992. 280p. $27.95.

The Urban Underclass. Edited by Christopher Jencks and Paul E. Peterson. Washington: Brookings Institution, 1991. 490p. $34.95 cloth, $12.95 paper.

The New Politics of Poverty: The Nonworking Poor in America. By Lawrence M. Mead. New York: Basic Books, 1992. 356p. $25.00.

Many sociologists and economists (but curiously few political scientists) rediscovered the poor during the past decade and are now turning out reams of reports, articles, and books about them. Arguments frequently recapitulate 1960s debates about "the culture of poverty" versus "structures of opportunity," and policy proposals are often depressingly familiar. But even if poverty is always with us, some things do change. African Americans now can, and frequently do, vote—in some places with powerful effects. Immigration from Asia and Latin America is rising, and immigrants' birthrates are high—so that long-time residents of the United States have many more non-Europeans to contend with and compare each other to. Statistics and the computers needed to manipulate them are vastly more sophisticated. Ethnographers are much more self-aware; policy proposals are sometimes subject to experimental manipulation; and large, longitudinal data sets are proliferating.

Both recapitulations and innovations are evident in these books, all three of which provide eminently worthwhile, if not always scintillating, reading. Two of the three focus on a small subset of the poor—the predominantly black, urban nonparticipants in mainstream society typically known as the "underclass"—and the first (*Rethinking Social Policy*) devotes considerable attention to the same group. I shall consider why so many devote so much attention to so few, but first a review of each book is in order.

The Urban Underclass consists largely of conceptual and (especially) empirical responses to William J. Wilson's *Truly Disadvantaged* (1987). Jencks challenges the view that the "underclass" is a coherent concept at all, and

J. David Greenstone provides a broad and subtle framework for thinking about the relationship between rationality and culture among the poor. The other essays focus more on a particular aspect of Wilson's opus. Reynolds Farley, for example, rebuts the claim that underclass problems are exacerbated by middle-class blacks' move to the suburbs by showing that blacks have always been more segregated by class than whites. Marta Tienda and Haya Stier distinguish between joblessness and shiftlessness and demonstrate that the so-called underclass experiences much more of the former than the latter. Jeffrey Berry, Kent Portnoy, and Ken Thomson show that in a few (atypical?) cities, poor blacks feel a greater sense of community, feel less politically efficacious, trust government more, and are just as politically active as nonpoor blacks, whites, and/or residents of nonpoor neighborhoods. Theda Skocpol reinforces Wilson's claim about the political and substantive value of promoting universalistic, rather than targeted, social policies and receives a lively rebuttal from Robert Greenstein. In perhaps the most innovative essay, Jonathan Crane makes a powerful case for the view that living in a very bad neighborhood is, by several orders of magnitude, worse than living in a pretty bad neighborhood. The rest of the 19 chapters address reasons for, and extent of, early child bearing, employers' views of poor black job seekers, factors that affect the likelihood of poor blacks' graduating from high school, the effects of tight labor markets on employment of poor blacks, and other critically important social and economic characteristics of poor blacks in poor urban neighborhoods.

Virtually all of the chapters are of a very high quality. Most rely on extensive and highly aggregated data sets; most are argued both carefully and powerfully. Thus, they seem—and to the best of my knowledge, are—authoritative. The few that approach the problem qualitatively, from the ground up, are a welcome relief. For my taste, the collection should have included more ethnography, as well as more politics; but any political scientist who wants to know what the best economists and sociologists are thinking about the urban underclass should start with this book.

For a clearer sense of the intense political and normative concerns that lie behind the dry presentations of *The Urban Underclass*, the questing political scientist would do well to turn next to Lawrence Mead's *New Politics of Poverty*. Mead has several great virtues. He is not afraid to speak directly of "the defeatist culture of poverty", and to draw out the implications of a conservative view even in the nest of liberals who usually write about poverty and social policy. He takes seriously, and is comfortable with, evidence of all sorts: political and policy histories, large data sets, even a little bit of testimony from poor people themselves. Most importantly, perhaps, he links his own and others' political values, poor and rich people's behavior, promulgated or proposed policies, and change over time. In short, he draws out one possible set of implications from the kind of material that Jencks and Peterson's authors delineate so thoroughly.

Mead makes three nested arguments. First, poor people are capable of working, and there are jobs available for them. If they would take these jobs, they would stop being dependent, and they would be less poor; but most are no longer willing to take the jobs available to them. Second, the consequences of this set of facts have profoundly changed the nature of American politics over the past few decades. Americans used to engage in "progressive politics," defined as debates over how to negotiate the competing claims of more- and less-well-off workers and their families; that is, when most of the male population worked for wages, political disputes revolved around the economic concerns of the majority. Should government be bigger or smaller? Should the tax system redistribute upward, downward, or sideways? How should we provide jobs for the few unfortunates who do not have them? Now, "the question of whether to enforce social values is more contentious. Dependency at the bottom of society, not economic equality, is the issue of the day" (p. ix); that is, Americans now focus a great deal of attention on controlling and resocializing the dependent poor who manifestly are not, and will not become, part of the working mainstream. Third, this shift in political focus from economic redistribution among the self-supporting majority to social control of the destitute minority has profound implications for Americans' sense of identity and direction. If people are able to take care of themselves, perhaps after a small boost, Americans can (correctly) think of themselves as autonomous, independent, and competent. If some cannot or will not take care of themselves, even after we have done everything possible to encourage or insist that they do so, Americans must recognize that at least some of us are weak and incompetent, forever needful of a paternalistic and controlling big government. In short, the debate over welfare dependency is a debate over the soul of America: "These very personal issues underlie the deepest disputes in domestic affairs" (p. xi).

As I read the evidence, Mead is wrong about most of the dependent poor. They want to work more than he gives them credit for. In fact, most do. He is also wrong in claiming that tension between the politics of redistribution and the politics of moral values is new. Americans have been fighting over that divide at least since Cotton Mather lamented about New Englanders who would rather fish than pray. But Mead is right about the most important thing. Well-off Americans have always used poor Americans (especially when they are of another race, as the Irish were considered to be in the nineteenth century) to define themselves and the worth of their institutions. And he is right that some poor people have simply dropped out or been pushed out of conventional channels of mobility, with devastating consequences for them and those with whom they come into contact.

Which brings us back to the urban underclass, and Jencks's *Rethinking Social Policy*. This book displays more fully the quality of Jencks's contribution to *The Urban Underclass*—his exquisite attention to detail and his ability to demonstrate why it matters, his political iconoclasm which makes for opponents on all sides, the clarity of his thinking and writing, and his rather frustrating refusal to pronounce on the big questions that Mead so eagerly takes on. *Rethinking Social Policy* collects and slightly updates six of Jencks's articles and review essays written during the past decade. It addresses affirmative action for middle-class blacks, the putative harmful incentives built into Aid for Families with Dependent Children, the genetic components of crime, the cultural components of black poverty, the need to disaggregate the "underclass" (the same essay as in *The Urban Underclass*), and the implications of the fact that most welfare recipients "cheat" by working. Each essay is self-contained; and all but the last are organized around a response to another social policy analyst, all conservative (Thomas Sowell, Charles Murray, James Q. Wilson) except for William J. Wilson. The essays are self-contained; and the book is probably better read as a series of distinct commentaries on important issues, rather than as a cumulative set of arguments. It has, in short, no overarching theme except the very important one that things are always more complicated than they first appear, so that any firm, ideological stance is partly (but probably not wholly) wrong.

The book is a pleasure to read. Consider, for example, the essay that begins with a survey showing that all of a small sample of single mothers on welfare in Chicago "cheat" by earning income that they do not report. After demonstrating the generalizability of these findings, Jencks shows how these "welfare cheats" earn and spend the money, describes what happens to those who do not earn, then asks, "Why don't more single mothers work?" To Murray, Lawrence Mead, and most politicians, he answers: "The essence of the 'welfare trap' is not that welfare warps women's personalities or makes them pathologically dependent, though that may occasionally happen. The essence of the trap is that, although welfare pays badly, low-wage jobs pay even worse" (p. 225). Welfare mothers will work when it is economically rational to do so and will not when it is not.

From this point (which is not unique to Jencks but is more carefully demonstrated than I have seen elsewhere), the essay moves on to political and policy implications. Here, too, the strength of the arguments lies not in their novelty but in their calm, sensible, almost irresistible logic. But we cannot peg Jencks as a smart but standard-issue liberal when we remember the earlier essays in which he challenges most strong versions of affirmative action, points out that black fathers at all income levels are decreasingly marrying the mothers of their children, and chastises liberal social policies in that they "often reward folly and vice" (p. 91).

We have, then, two hedgehogs and one fox among these books. The fox makes the most exciting but also most dubious arguments; the hedgehogs are hard to challenge but are also less likely to make our students wake up and argue. In combination, these books make me wish for more of three things: more representation by the poor themselves in discussions about them, more participation by political scientists in these debates, and more self-consciousness about why jobless urban blacks receive so much more attention than Appalachian moonshiners in our discussions of race and class in the United States.

Princeton University JENNIFER L. HOCHSCHILD

Congress and Nuclear Weapons. By James M. Lindsay. Baltimore: Johns Hopkins University Press, 1991. 205p. $29.50.

This book attempts to ascertain the bases of congressional decisions on nuclear weapons acquisition. *Congress and Nuclear Weapons* also addresses, as secondary concerns, the degree of congressional influence on nuclear arms policy and the quality of congressional decisions. Lindsay compares three different explanations for why Congress acts as it does on nuclear procurement matters. He largely refutes two explanations traditional in the political science literature and popular conceptions. Lindsay argues that Congress is no longer driven by a norm of deference to the executive branch, nor is its decision making motivated primarily by parochial concerns. "What best explains congressional decision making are the personal policy preferences of individual members" (p. xii).

This work is well written and clearly organized. One of its strongest features is the use of multiple forms of evidence. Lindsay refers to a range of relevant statistical studies. He also includes four case studies of individual weapons system decisions and draws on some 75 interviews he conducted with members of the Washington community.

Lindsay's argument is strongest when he discusses the inadequacies of the perspectives he seeks to refute. He makes a compelling case for rejecting the idea that Congress has no impact on policy. Lindsay is also persuasive on the limitations of approaches that emphasize pork-barreling or "bile-barreling" (a rather inelegant term for what is more commonly referred to as not-in-my-backyard [NIMBY] politics).

Unfortunately, the argument that congressional decision making on nuclear weapons acquisition is driven by policy concerns is not developed fully enough to advance our understanding very far. No systematic discussion is given to the *context* in which Congress makes its decisions. Yet surely, trends in public opinion or interest group activity and developments in America's international environment are likely to affect members' attitudes about appropriate nuclear policy. At times, Lindsay notes the role played by a large-scale grassroots lobbying effort or changes in the level of U.S.–Soviet hostility (pp. 104, 125–26). But these are not introduced as factors to be considered routinely in the statistical and case study analyses he conducts. As a result, the possibility that the state of U.S.–Soviet relations or the level of organized citizen activism accounts for variations in congressional resistance to administration defense requests is not considered. This makes it difficult to ascertain whether policy preferences are simply a surrogate for these other forces outside of Congress or something more primordial (as Lindsay seems to want us to believe).

Lindsay does devote some discussion to why certain nuclear weapons rise on the congressional agenda and become subjects of a major dispute with the administration, even though most weapons requests are routinely approved by Congress. He argues that controversial systems are distinguished from weapons that win approval easily by either or both of two attributes: (1) a decision to produce the weapon would cause a break with existing declaratory doctrine, or (2) it would make the United States the first to cross some new threshold in weapons development (pp. 99–101).

But I suspect that Lindsay has identified as a cause of congressional intervention a rationale with which opponents of an administration request can build a coalition large enough to challenge the president's policy seriously. For example, Lindsay argues that the reason that Congress cut back executive branch plans for MX procurement while tinkering very little with the Trident D-5 program was that proposals to place the MX in silos violated the tenet of survivability, which was not a problem for the sea-based Trident missile. But initial opposition to the MX came primarily from legislators who feared that the missile's accuracy threatened stability by reducing the survivability of *Soviet* forces and also from an extensive, well-organized grassroots campaign against the weapon. Once this put MX on the congressional agenda, both Carter and Reagan's inability to find a basing mode that was both environmentally acceptable and survivable provided an argument that helped bring moderate-to-conservative legislators into the anti-MX camp, as well. Thus, Lindsay is right in identifying this departure from existing nuclear doctrine as a factor that put the MX in danger of cancellation, but he is not right to see this as the primary reason that the system got onto the congressional agenda in the first place.

I believe *Congress and Nuclear Weapons* would make an excellent text for an upper-level undergraduate class on Congress or on U.S. foreign policy formulation. It should also be brought to the attention of nonscholarly audiences, because it contains a first-rate discussion of why critics of congressional involvement in nuclear weapons policymaking have exaggerated the problems and overlooked the benefits of such involvement.

Lindsay's scholarly contribution is limited, however, by the somewhat narrow scope of the book. Although he notes that its topic falls at the intersection of the traditionally separate subfields of American politics and international relations, he draws very little on the literature and ideas of the latter subfield, relying almost entirely on the literature on Congress. Lindsay also explicitly excludes nuclear arms control from his purview. Yet inclusion of some arms control cases might have permitted a more fruitful discussion of relations between Congress and the larger domestic political environment. For example, examination of congressional response to the nuclear weapons freeze campaign in the early 1980s might have improved and strengthened Lindsay's inferences regarding how electoral and policy incentives interact.

Within the framework and parameters he sets up, Lindsay's analysis is sensible and hard to fault. Lindsay makes a good case for considering Congress a significant player in defense policy, as well as domestic policy. *Congress and Nuclear Weapons* also suggests that the importance of the electoral connection relative to policy motivations may have been exaggerated in the previous literature. This book could have done more, however, if it had devoted more attention to the public and international context within which Congress acts and explored the full range of nuclear weapons decisions, both procurement and arms control.

University of Southern California JEFFREY W. KNOPF

The Structure of Corporate Political Action: Interfirm Relations and Their Consequences. By Mark S. Mizruchi. Cambridge: Harvard University Press, 1992. 299p. $37.50.

Two paradigms encompass most existing work on political action by corporations. The *institutional political science* approach emphasizes the incumbent targets of corporate political action on specific issues. The explanatory variables in these analyses are institutional features such as committee assignments and chairmanships, seniority, and electoral security. The *economic* approach emphasizes narrow self-interest. The rational model explains corporate contributions based on the ability to act on shared interests and avoid free-riding. The key explanatory variables in this work have been firm size, industry structure, and government purchases and regulation.

Mizruchi's book takes a very different approach, one that harkens back to Bauer, Pool, and Dexter's *American Business and Public Policy* (1972). The focus is on how networks of interaction among firms influence their political activities. The questions that motivate Mizruchi's work at the outset derive from his background as a sociologist. The first chapter is an engaging and cogent discussion both of the literature on pluralism and elitism, and of Mizruchi's own intellectual evolution. The central argument relates to the classic debate on the role of interest groups in a democracy.

Elite theorists base their claims on the existence of a shared interest among ruling groups, particularly owners of capital. Pluralists agree that interest gives impetus to action but argue that few interests are broadly shared and that cross-cutting cleavages divide the classes. For example, workers and owners of an export-oriented firm share economic interests that may conflict with the interests of counterparts in an import-competing firm. The more powerful interest determines policy, *on any one issue*. But pervasive and cross-cutting cleavages mean that coalitions that form trade policy differ from those that form domestic social policy, foreign policy, and so on. No single, ruling elite maintains the overall social order, either instrumentally or structurally.

Elite-theorists respond that elites do differ but that such conflicts are largely solved by *mediating mechanisms* evolved within the business community. Mizruchi notes that this point has long been made conceptually but that "incredibly, . . . except for a small number of studies, . . . elite theorists and those sympathetic to elite theory did not . . . demonstrate the ways in which these mechanisms in fact led to business conflict resolution" (p. 6). The mechanisms on which Mizruchi focuses (as in his *American Corporate Network* [1982]) are relations among manufacturing firms, including policy-planning organizations, trade associations, and interlocking directorates. Mizruchi's research agenda is to demonstrate that if interlocks and other mediating mechanisms can be shown to have pervasive behavioral consequences, the pluralist position has no real answer to the fact that they exist broadly.

The theory is tested on a sample of 57 large U.S. manufacturing firms in the early 1980s. The primary dependent variable in this analysis is the pattern of campaign contributions by firms, although Mizruchi also explores the extent to which firms act similarly in testimony before congressional committees. The goal of the analysis is to demonstrate that narrow, short-run economic self-interest is a less important explanation of the pattern of political activity than the organizational and social network in which the firms are placed. Mizruchi claims to be able to draw this conclusion from his data. I expect that most political scientists will be somewhat put off by the empirical sections of the book, partly because of the differences in terminology and methods in sociology but also because the analysis is so devoid of institutional political focus. Nonetheless, as an insight into the pattern of organization of political action committees by firms and industries, *The Structure of Corporate Political Action* is very valuable.

Problems arise with the broader conclusions that the book seeks to draw, for two reasons. First, the reasoning is inductive: the pattern of interlocks has *some* behavioral consequences (similarities in the pattern of political activities). But the material policy consequences are never demonstrated. The political science literature on organizational impulses and goals of corporate political action committees renders this conclusion questionable. For example, Wright concludes that political action committees may be more interested in trying to attract donations from employees than in pursuing some coherent political goal ("PACs, Contributions, and Roll Calls," *American Political Science Review* (1985). If this is true and corporate political action committees face similar groups of employees, it is quite reasonable to expect similarities in contribution patterns. It is unjustified, however, to conclude from these similarities that any coherent external political goals are being pursued.

Second, the overall explanatory power of the statistical analysis is feeble. The R-squares in the table describing corporate campaign contributions are never higher than .206. The tables explaining the similarities in business testimony before Congress are even worse, with no regressions doing better than .069. Many of the individual explanatory variables that Mizruchi advances (interlocking directorates, industry, etc.) are statistically significant. But these "significant" effects are substantively trivial.

Nonetheless, *The Structure of Corporate Political Action* synthesizes some of the most prepollent questions in the social sciences. The two criticisms offered here—the indirectness of the argument and the overall explanatory weakness of the model—are likely to be simply unanswered questions in the research agenda that Mizruchi has suggested in a genuinely new perspective on corporate political activity.

University of North Carolina,
Chapel Hill
Michael C. Munger

Emerging Influentials in State Legislatures: Women, Blacks, and Hispanics. By Albert J. Nelson. Westport: Praeger, 1991. 168p. $39.95.

For students of representation, legislative behavior, public policy, and state politics, *Emerging Influentials in State Legislatures* by Albert J. Nelson provides a wealth of data on (1) the factors influencing female, black, and Hispanic representation in state legislatures; (2) how far members of these groups have achieved positions of potential legislative influence; and (3) their impact upon public policy. This is a timely study because the ranks of women and minority state legislators have grown and can be expected to increase still more in the 1990s. The analysis is well grounded in prior research in the field, and the author draws upon this research effectively to frame hypotheses and provide explanations for his findings. The importance of Nelson's work is enhanced by its unique features: longitudinal analysis (1982–86 elections), separate analysis of Republican and Democratic legislative parties, attention to turnover, and analysis of the influence and policy impact of female and minority legislators.

Nelson analyzes 45 state legislative lower chambers. (Nebraska's unicameral legislature is omitted, as are the legislatures of Alabama, Louisiana, Maryland, and Mississippi because their lower-house members are elected for four-year terms.) The principle dependent variables, which Nelson labels "measures of incorporation," are (1) the mean percentage of Democratic and Republican females (other than Hispanic and blacks), Hispanics, and blacks, represented in the chambers; (2) the mean percentage of turnover of these groups within chambers; and (3) indices of potential intrachamber influence. Twenty-four independent variables relating to state demography, party structure, and legislative organization (e.g., salary, committee system) are employed. To assess the impact of the measures of incorporation upon public policy, Nelson utilizes three policy variables: per capita expenditures for education, social services, and health and hospitals.

The result is an impressive array of data and findings that scholars in the field will wish to examine and test more fully. Many of the findings confirm expectations. For instance, female representation is inversely related to the proportion of conservative Protestants and Roman Catholic residents within a state and positively related to the proportion of women working within a state. Other findings run counter to expectations. For example, Nelson did not find a strong relationship between Democratic female and minority representation in lower chambers and per capita expenditures for education, social services, and health. He did, however, find that increased expenditures for education and social services (but not health) were strongly related to female representation when there was unified Democratic control of state government (pp. 115–17). But in state governments under unified Republican control, there was a significant and inverse relationship between female legislative influence in lower chambers and per capita expenditures for education, social services, and health. Findings such as these underscore the validity of Nelson's decision to analyze the Republican and Democratic legislators separately and point up the importance of partisan influences.

Although this volume is filled with significant and intriguing findings, it does have its limitations. One, which the author readily acknowledges, is the fact that all of the various demographic and party structure variables are statewide measures. This level of analysis distinctly limits the utility of the research because it makes it extremely difficult to determine with exactitude which factors are actually contributing to the election of female and minority legislators in specific constituencies. For example, the fact that the proportion of the population residing in metropolitan areas is positively related to female and black representation for both Republicans and Democrats contributes little to our understanding unless we make assumptions that are not substantiated by the data (e.g., that white female legislators represent suburban areas [p. 135]). Reliance on statewide measures also produces some anomalous findings, for example, an inverse relationship between the proportion of the state population that is Roman Catholic and Hispanic representation (p. 44).

The reader also encounters some minor distractions, including redundant comments about the ambiguity of findings and clichés such as "reading tea leaves" of future political history. However, this book does merit the attention of students of state legislatures and female–black–Hispanic representation because its findings provide an array of hypotheses that require further analysis.

University of Wisconsin, Milwaukee
John F. Bibby

Make-Believe Media: The Politics of Entertainment. By Michael Parenti. New York: St. Martin's, 1992. 241p. $17.35 Paper.

In this clearly written, brief book, well-known social critic Michael Parenti expresses his grave concerns about the impact of the entertainment media on the perceptions of politics held by average Americans. Parenti cities a number of findings from the social science literature to substantiate that the danger of "mind pollution" is real—without, however, indicating that the conditions in which these findings apply are limited.

What kinds of images do profit-seeking media producers provide for their gullible audiences? An eclectic content analysis of entertainment fare has convinced Parenti that much of it is stereotypical garbage. Stereotypes range from Third World people of various skin colors depicted as savage, inferior beings who deserve subjugation and even extermination to glorification of violence, greed, and consumerism in shows designed for children. The impact is disastrous.

The images and themes dispensed by Hollywood and the television industry spread detrimental ideologies, distort reality, lull people into a false sense of security, and dull their critical faculties. Prevalent images support imperialism, rabid anticommunism, and exploitative capitalism. They enhance racism directed at nonwhites, exalt sexism, and glorify militarism, violence, and a multitude of other social and ideological sins. The entertainment format makes political propaganda all the more insidious. Since entertainment stories make no claim to being real, audiences put their critical faculties on hold. They passively absorb what is presented to them, blurring the distinctions between real life and fictional presentations in their minds. Make-believe pretending to mirror real life *makes* audiences *believe*.

Parenti acknowledges that people do not believe everything that they see and hear, particularly when it runs counter to what they have been taught and have actually experienced. But for many situations, they lack real life models that would permit them to put the entertainment presentation into perspective. Parenti also acknowledges that there are movies and television shows that avoid the stereotypes that he condemns and that disseminate the left-wing ideology that he prefers. In fact, he claims that his content analysis was skewed toward these types of productions. Nonetheless, the dross of commercial entertainment garbage overwhelms the constructive messages that are the beacons of hope in the current electronic wasteland.

The problem with Parenti's argument is that media content, entertainment or otherwise, is polysemic. People can and do assign different meanings to what they see and hear. When an airport scene depicts a black porter carrying a white business man's bags, one may regard that as stereotyping of blacks as inferior servants of whites, or one may view it as merely an example of a commonly encountered scene, devoid of any intimations about the social status of different racial groups. A picture of soldiers cheering an American flag overseas may be taken as evidence of noxious chauvinism and imperialism, or it may be regarded as a refreshingly patriotic theme.

Depending on the ideological spin that various readers put on their interpretations of the scenes and events shown in entertainment shows, they are likely to hail or condemn Parenti's recommendations for changing American entertainment fare. These include recommendations to boycott the kinds of movies and television shows that Parenti condemns and to organize pressure groups that might persuade media owners to mend their errant ways. Parenti also favors creating more noncommercial, publicly financed television and movie making, portraying a much broader, more left-wing array of documentaries, docudramas, and movies.

This is a thought-provoking study that forces the reader to take sides. How valid are Parenti's interpretations of meanings of images and overall image trends? How representative was the sample from which his illustrations were drawn? How strong and pervasive is the political impact of entertainment fare? Totally objective answers to such questions remain elusive. Parenti has once again stirred the cauldron of controversy, but he has brought us no closer to a resolution.

University of Illinois, Chicago DORIS A. GRABER

The Politics of Interests: Interest Groups Transformed. Edited by Mark P. Petracca. Boulder: Westview, 1992. 421p. $58.50 cloth, $19.95 paper.

Mark Petracca asserts that *The Politics of Interests* "has been organized and written to appeal to undergraduates" (p. xxiv). However, this volume, while valuable in many ways, is simply not well suited to most undergraduate interest group courses. Rather, its value lies in providing some broad perspectives that integrate thorough bibliographic work with some findings from original research. This would be an excellent book for a graduate student to read in preparing for preliminary exams or for a professor preparing for a new course on interest groups.

Petracca's introductory essay provides a reasonable roadmap through the numerous strains of interest group politics, from the nineteenth century through the present, although his omission of Margaret Thompson's fascinating Gilded Age study, *The Spider's Web* (1986), is truly puzzling. Petracca and his contributors focus their attention largely on membership groups. This is problematic for at least two reasons: (1) many groups are funded by patrons, although we know relatively little about how or why; and (2) many interests, even "organized interests," are structured outside the group format. Universities, think tanks, and corporations are all organized interests without being interest groups. To be sure, Petracca argues that the volume is not meant to be comprehensive; but its encompassing title would seem to be at odds with that premise.

Turning to the essays, they are a mixed bag. In an interesting juxtaposition, Jane Mansbridge extends her consideration of deliberation to groups, while Andrew McFarland follows with a piece that emphasizes groups' traditional behavior as "countervailing power." Overall, only one article—Sabatier's on local groups—engages ideas drawn from the rich literature engendered by Mancur Olson. Even Sabatier's test of Olson's findings is essentially a single, limited, local case. For a book that seeks to address undergraduates, the calculus of group membership deserves more attention.

In perhaps the strangest piece, Paul Peterson finds a sharp decline in the influence of special interests during the 1980s, a decade when various observers have asserted that interests had both proliferated and become more influential. Peterson makes a macro argument that such interests, while growing in numbers, have competed for a declining share of discretionary spending in the 1980s. This provocative argument might have proved the beginning of an important debate save for the fact that Peterson defines special interests so narrowly (e.g., defense contractors and farm groups do not quality) to render the concept almost meaningless. In addition, Peterson does not discuss the lobbying that surrounds regulation or tax issues.

Running in the opposite direction, John Tierney argues that "more of the same" in Washington lobbying "is not really the same"; rather, "where there was potential for Madisonian mischief before, there is now a potential for long-term harm" (p. 219). As editor, Petracca does not sort through the disagreements between Peterson and Tierney. Nor does he work through the contradictions of the Mansbridge and McFarland pieces. Indeed, there is real potential for recasting some of the major debates in interest group politics; but most of the essays move off on their own, cataloging specific trends in consumerism, the women's movement, direct mail, and the like.

In this vein, the collection does provide the reader with a fairly good indication of the varied directions in which the fragmented field of interest group politics is headed in the 1990s. It also demonstrates that there is no clear theoretical focal point for understanding the politics of interests. In 1982, Douglas Arnold could accurately observe that the interest group field was theory-rich and data-poor. This is (as Petracca notes) no longer the case, with various major studies around, including those of Tierney (with Schlozman) and Salisbury and his colleagues (both groups represented by articles here) and many other empirical efforts. The theoretical unity that Olson might once have appeared to provide has been shattered. Much like contemporary interest group politics, the study of interests is atomized, with niches existing here and there (the role of business, public interest groups) and major questions going begging (e.g., the overall calculus of lobbying). Still, *The Politics of Interests* sketches out the range of group-based studies; and Petracca's final chapter does call for more data about more types of groups, more comparative studies, more analyses of groups in electoral politics, and (after Mansbridge) more attention to interests as deliberative forces.

In the end, while this book is valuable and suggestive, it will not serve to reorient a field that has been notoriously difficult to pin down over the past decade. Perhaps that is too tall an order for a single volume, but the "politics of interests" surely is in need of some such refocusing. If not offering a road map, this collection does send us off on some interesting, and possibly rewarding, journeys.

University of Kansas BURDETT LOOMIS

The Right to Privacy: Gays, Lesbians, and the Constitution. By Vincent J. Samar. Philadelphia: Temple University Press, 1991. 254p. $39.95.

Abortion Rights as Religious Freedom. By Peter S. Wenz. Philadelphia: Temple University Press, 1992. 294p. $49.95 cloth, $22.95 paper.

Both these volumes are excursions in the philosophy of law. The Samar book offers a provocative interpretation, while the Wenz book presents a compelling analysis of the abortion problem and an alternative scenario by which the Supreme Court might nurture a policy consensus. Wenz correctly views the abortion controversy as largely resulting from the unsettled, nontextual and open-ended privacy doctrine that the Supreme Court embraced in 1973 but that has given way to inconsistencies and the erosion of principle in the progeny of *Roe* v. *Wade*. Why? Because, he argues, privacy lacks the textual or historical grounding to be a guiding principle of the Constitution.

Samar, in contrast, would extend the concept of privacy to justify consenting homosexual relations among adults. Although the Supreme Court upheld a Georgia sodomy law in 1986 (*Bowers* v. *Hardwick*), Samar asserts that the ruling "was mistaken" and that the Court "should at its earliest opportunity reconsider that decision" (p. 172). To overturn that 5-4 decision now is not likely, since, given the changing personnel of the Court since then, any reconsideration might well yield a 7-2 margin. Citing Fourth Amendment cases, tort law, and constitutional precedents on procreational rights, Samar derives a "theory of privacy that explains precisely what privacy is and shows how it is justified": "Because democratic government in the Western sense values autonomy as a fundamental end, it must value privacy as a fundamental right" (p. 205). Individual "autonomy" is not a negative, but a positive, freedom that obliges government to promote privacy in order to fulfill democratic ends. But Samar's argument falters on this conceptualization. How can democratic values be used to defend an extreme libertarian view when a kind of majority tyranny lies at the heart of the problem of securing gay rights?

Also, the Samar book advocates a position with tremendous conflict potential at a time when abortion has become more politicized than it was in 1973. Perhaps concerns about organizational maintenance prompted the high court in 1986 *not* to follow the Samar line of argument by extending the privacy guarantees of the *Griswold* and *Roe* precedents to gay life-styles.

A more viable alternative, if we take the thesis of the Wenz book to heart, would be to justify gay rights through the equal protection clause of the Fourteenth Amendment, rather than pursue a rearguard battle over a discredited privacy doctrine. Equal protection is not considered in the Wenz analysis however, since it may be a more dubious prochoice strategy than another alternative that he cleverly lays out. Wenz believes that freedom of choice on abortion (if not abortion rights!) can be defended on church–state grounds because this jurisprudence is grounded in the history and meaning of the religion clauses, and our political culture accepts the tenets of nonestablishment and neutrality. Wenz, unlike legal purists, understands the requirement of consensus building for any public policy to endure and for any judicial decision to retain its political legitimacy.

Wenz rejects both the "extreme Conservative view, [which] fails adequately to portray the Constitution as reflecting the ideals that we expect it to embody," and the "extreme Liberal view, [which] fails adequately to satisfy the criterion of congruence with the Constitution and with its interpretation by the Supreme Court" (esp. substantive due process as viewed by Tribe in *Abortion: The Clash of Absolutes* [1990]), in favor of a "Moderate view [that] would confine the constitutional protection of individual rights to rights explicitly mentioned in the

Constitution"—and "in their relatively specific, rather than general, form"—but also "allow broad scope to the development of the meaning of the specific concept in question" (pp. 10, 12–13).

Thus, Wenz believes that the establishment clause and the free exercise clause are more than adequate to deal with the abortion problem and concludes that in that case, most, but not all, decisions by the high court on abortion cases (through *Webster*) would come out the same, while a more coherent legal reasoning would have guided the opinions of the justices. Here, Wenz distinguishes between *religious* beliefs, which "cannot be established by appeal merely to secular premises and methodologies," and *secular* beliefs, which are "agreements of belief, thought, and practice that are the basis of the cooperation and mutual understanding" (p. 112).

Justice O'Connor is correct, he believes, in asserting that the *Roe* trimester framework is on a collision course with itself because medical technology has pushed safe abortion past the first trimester and viability before the third trimester. But O'Connor's concern for "potential" life is misconstrued, says Wenz, who substitutes "similarity to newborns" as the constitutional standard. Fetuses 20 weeks or younger "are so dissimilar to newborns that attributions of personhood to them are epistemologically religious" (p. 197), whereas beginning with the twenty-first week, the unborn's essential humanity is a secular matter. This kind of division better accommodates future advances in medical technology and, more important, comports with long-standing Court precedents respecting religious liberty. In sum, anticontraception and antiabortion laws applicable before the twenty-first week violate the establishment clause of the First Amendment.

However there is a pitfall to the Wenz position. Moral conflicts polarize, and his settlement of the abortion problem may give rise to another round of sectarian debate. If abortion expresses a religious belief in violation of the establishment clause, what of "In God We Trust" on the nation's money supply? Certainly, the Court might view the issue as purely symbolic (unlike abortion policy) and not enter that political thicket. But the problem is that while the religion clauses are not as open-ended as the concept of privacy rights, Wenz does not imply any real limits to the judicial reach of his doctrine.

As both Samar and Wenz focus on legal concepts and precedents to weave their constitutional arguments, neither provides any empirical support to validate the assumptions implicit in their reasoning. The Supreme Court does not operate in a political vacuum, and this kind of scholarship should try to integrate social science research and opinion data to indicate the degree of public acceptance of gay rights or religious liberty. A critical factor affecting compliance with Supreme Court rulings is whether those decrees have any degree of support among community elites or from the citizenry. The absence of this documentation especially weakens Wenz's effort at a consensus position on abortion, because he asserts without hesitation that our social values largely affirm the Court's understanding of nonestablishment and neutrality in matters of religion.

The Samar volume might be a useful resource on the law affecting gay rights. The Wenz volume is a valiant effort to forge a middle position in the seemingly intractable conflict between prochoice and prolife forces. It ought to be required reading for anyone interested in abortion politics and the law.

Loyola University, Chicago RAYMOND TATALOVICH

Questions about Questions: Inquiries into the Cognitive Bases of Surveys. Edited by Judith M. Tanur. New York: Russell Sage Foundation, 1992. 306p. $34.95.

The sample survey has deeply influenced the study of the American political process (and the American political process itself). However, a certain reevaluation of the meaning of survey data is evident in the spate of think pieces appearing in political science journals and op-ed pages pointing out that the disparate poll results that have become a staple of campaign coverage are the result of differing survey methods (e.g. question wording and question order). While our discipline has prided itself on growing sophistication on these matters, the sample survey enterprise has been undergoing far more searching, indeed radical, reconsideration from linguists, anthropologists, and cognitive psychologists. Instead of asking about the effects of changes in question wording or alterations in context for a particular question battery, the inquiry has expanded into the "cognitive processes that underlie the process of asking and answering questions." Understanding the survey from this perspective, it is argued, can lead to a better handle in particular response errors.

The movement to study the cognitive aspects of survey methodology was given impetus by serious questions that arose about the validity of factual recall data gathered in the federal governments large-scale longitudinal surveys (e.g., the National Crime Survey and the National Health Interview Survey). There is now a large literature in the area and no less than three "cognitive laboratories" housed in government agencies.

The volume begins with a useful introduction and brief history of this movement. It is bracketed by two articles that treat the survey interview from a linguist's perspective. We are reminded that a survey interview is one particular use of language, which (like all language use) is "essentially about speakers' intentions—what speakers intend in choosing the words they do and what their addressees take them as intending. Once we understand the role of speakers' intentions in language use, we will find many of the problems of survey design more tractable" (p. 15). The second of the two linguistic articles, by Suchman and Jordan, reports on ethnographic analyses of videotaped survey interviews. Their summary finding is that "the validity of survey data is potentially undermined by the same prohibition against interaction that is intended to ensure reliability." We instruct survey interviewers not to deviate from what is on the printed page. We train interviewers in excruciating detail how to answer requests from the respondent for clarification of question meaning. Often, interviewers are only allowed to say, "Whatever it means to you." We do this in hopes that we are thereby providing a standard stimulus. The gist of the work of these two scholars is that this hope is actually ill founded and that validity and reliability must rest on standardized *understanding* by our respondents of the words we use. They argue that interviewers must be allowed to collaborate explicitly with respondents in figuring out the actual intention of the question and how it applies to them.

This is pretty heady stuff. The more sweeping assertions of Suchman and Jordan are probably best read as opening salvos in what we can hope will become a carefully measured reconsideration of standardization in the survey interview, informed by field experiments. Meanwhile, the wider debate would have been better served if the Suchman and Jordan article had been accompanied by some of the judicious commentary it received when it first appeared in 1990. It was pointed out, for example, that concern with standardization does not result from mindless concern with scientism but from real experience with interviewer bias effects when question wordings are *not* standardized.

Most of the remaining articles report on the current state of research in the well-known problems of accurate recall and reporting of factual data in surveys. The literature in this area is quite extensive, and this is an interesting sampling. Readers who wonder about the relevance to political data have only to think of the substantial recall-and-reporting component in the National Election Studies time series surveys (e.g., media attentiveness, campaign contributions, contacts with congressional incumbents, past party identification, past voting behavior, last year's income, retrospective economic evaluations). Should these measures be looked at anew, so that revised measures that do a better job of getting at the research objectives can be carefully calibrated into the surveys? Very possibly, but before wholesale redrafting is undertaken, one would want some demonstration that there will be payoff in data analyses. For example, in an article on overreporting of the vote, reference is made to misreporters as "polluting the pool of apparent voters." However, it is still an open question how far filtering out this "pollution" would improve most analyses of vote choice. This literature, as reflected in this volume, is weakest in linking measurement problems to data analysis.

The remaining section consists of two well-written and useful pieces on attitude measurement. Although they are well worth reading on their own and ingeniously folded into place as "expressions" of what is recalled from memory, it seems to me to be stretching a point to include them in a volume on the cognitive bases of the survey.

We know survey respondents only in summary statistical fashion: few of us have actually conducted our own interviews. It is all too easy to forget that the quantities plugged into equations result from complex human interactions that follow rules and logic quite a bit broader than the simple stimulus–response model that we seem to carry in our heads about what goes on in survey interviews. Other recent works (e.g., Biemer and his colleagues' *Measurement Error in Surveys*, 1991) are considerably more sophisticated methodologically; but this volume provides an interesting and accessible introduction to a growing literature about a major research instrument in our discipline.

University of Michigan SANTA TRAUGOTT

Progrowth Politics: Change and Governance in Houston. By Robert D. Thomas and Richard W. Murray. Franklin K. Lane Series on Governance in Metropolitan Areas, Vol. 9. Berkeley: Institute of Governmental Studies Press, 1991. 428p. $19.95 paper.

Previous volumes in this series have focused on London, Toronto, Stockholm, New York, Winnipeg, Indianapolis, Montreal, and Leningrad. The studies have shared the considerable strength of being rich in data and comprehensive in approach, although, in some instances, the descriptive material has overwhelmed the theoretical analysis. The Houston study is a first-rate contribution, setting a very high standard of comprehensive historical and political research woven skillfully into the literature on urban politics.

Although the table of contents encompasses many standard topics (a demographic profile of the region, the politics of annexation and state–urban and national–city relations, planning and zoning, growth politics, etc.). Thomas and Murray precisely situate Houston's politics within themes of the theoretical literature. They are particularly successful in describing how Houston's brand of growth politics compares with progrowth politics in other cities. Although the authors do not make self-conscious claims to new theoretical insights, their book constitutes an important contribution to the literature on growth regimes.

Thomas and Murray begin their discussion of growth politics (somewhat conventionally) by referring to Paul Peterson's and Clarence Stone's contributions. Rather than referring to the differences between these two, however, they borrow perspectives from both. From Peterson, they apply the notion that local decision makers are bound by the limited choices before them to promote the economic development of the local economy. Accordingly, Thomas and Murray promise to describe the external influences and constraints affecting Houston's economy and to evaluate the success of growth policies in keeping Houston's economy competitive. From Stone they take the cue that local growth coalitions (they eschew the term *regimes* without explaining why) change over time and that growth strategies reflect the composition of these coalitions. Thomas and Murray do a splendid job of showing how external forces and constraints interact with the internal politics that determine how Houston defines its policy responses and priorities.

Three topics illustrate the authors' careful blending of theoretical concept and descriptive material. In their discussion of special district politics they show how in Houston, the local state has been constructed as a straightforward expression of the needs of developers and business leaders, not as an expression of democratic governance. Their detailed history of the creation of special districts is interesting on its own; but this material adds up to a story with real substance when the authors show how special districts have constituted a perfect reflection of Houston's political culture, wherein local government has been constructed to prevent or channel participation and protect elite interests.

Their treatment of racial and ethnic issues is particularly good. They begin by situating Houston within V. O. Key's one-party *Southern Politics*, with restricted suffrage and discriminatory political arrangements. Then they present a detailed account of how the Voting Rights Act (an external pressure) was applied to change local politics, resulting, ultimately, in reforms of the at-large city council and the enhanced representation of women and minorities in Houston's elections over the past two decades.

Anyone interested in contemporary planning politics should read Thomas and Murray's account of Houston's long-time refusal to enact zoning. Briefly, they persuasively demonstrate that planning and zoning were never before on Houston's agenda not only because they were regarded as socialist measures but because they did not seem necessary for local economic growth. The recent, reluctant decision by the city to phase in zoning demonstrates just how intimately ideology is tied to self-interest. The new entrepreneurs have carried the day by arguing the practical point that future growth demands some attention to environmental concerns.

Thomas and Murray do not break new ground in developing theoretical insights, and such is not their goal. Their book comes close to being a model of how to use the theoretical insights of others to guide careful empirical work.

University of Missouri, St. Louis DENNIS R. JUDD

Urban Political Economy: Broward County, Florida. By Ronald K. Vogel. Gainesville: University Press of Florida, 1992. 176p. $24.95.

Welcome to the sun and fun capital of the world, where economy and government are so fragmented that both businessmen and politicians yearn for a power structure. Hyperpluralism reigns, and no one is very happy about it. Civic-minded people are frustrated: "There is no leadership in Broward. . . . People would follow if there were a leader" (p. 47).

Growth (particularly, rapid growth) breaks down older community power structures. Broward County and its 28 municipalities, including the cities of Fort Lauderdale, Pompano Beach, and Hollywood, tripled its population between 1960 and 1990. This growth came on top of a decentralized governmental structure and (perhaps more importantly) fragmented the economic structure. With both central components of the "urban political economy" lacking cohesion, the result is an inability to make policy: "Hyperpluralism disadvantages communities in their efforts to attract growth and capital" (p. 124).

But Ronald K. Vogel's insightful reporting on the political economy of a South Florida county is more than a case study. It is also a careful review of previous writings on political and economic power in communities and the development of a typology of community regimes. The notion of "political economy" encourages Vogel to view political and economic structures as separate components of a regime, each of which may be centralized or decentralized. The result is a four-celled typology of regimes: *cooperative* (both centralized), *economic elite* (economy centralized, polity decentralized), *political elite* (polity centralized, economy decentralized), and *hyperpluralism* (both decentralized). Vogel offers this typology as an improvement over the "simplistic" classification of communities as elitist or pluralist.

Hyperpluralism leads to deliberate efforts on the part of community leaders to centralize power, both economic and governmental. Economic elites, disgusted with the inability of fragmented government to deal with infrastructure development, seek to organize themselves and streamline government. Even the newspaper has "an interest in bringing the county together as a market" (p. 73). But such efforts meet with only limited success: "In Sunbelt cities, it would seem that growth with increased diversity and social pluralism leads not to just pluralism (which seems itself to be a transitional phase) but to hyperpluralism" (p. 120).

Vogel's analysis of Broward County's power structure appears to be based on extensive interviews of leaders in business, government law, education, and the media and buttressed by newspaper accounts of important community decisions. Vogel includes a standard reputational identification of powerholders that produces multiple nominations of bankers, developers, utility executives, Congress members, county commissions, and state legislators.

Vogel is on shakier grounds in his conclusions, where he attempts to place other cities into his fourfold categorization, an effort he describes as "a speculative venture." Nationwide, Vogel foresees "the passing of economic elites," as well as the decline of political elites. Instead, "most communities today strive to reach the cooperative pattern of community decision making" (p. 121), with an emphasis on public–private partnerships. Yet Vogel's own study illustrates the many obstacles to achieving the cooperative status. Moreover, we might ponder Vogel's apparent bias toward what others would label "corporatism." Hyperpluralism may be frustrating, but is corporatism the solution?

Florida State University THOMAS R. DYE

The Fractured Metropolis: Political Fragmentation and Metropolitan Segregation. By Gregory R. Weiher. Albany: State University of New York Press, 1991. 225p. $12.95 paper.

In this engrossing analysis of the seemingly arcane topic of political boundaries, Gregory Weiher makes an important and timely statement about the spatial context of citizenship. As Weiher's study makes clear, urban theorists are inattentive to the geographic dimensions of fragmentation, despite metaphorical references to "the urban mosaic," "the fragmented city," and now "the edge city." By focusing primarily on the efficiency and equity consequences of metropolitan fragmentation at the institutional level, scholars have "overlooked the obvious," namely, how political boundaries in and of themselves serve as information systems allowing those with resources to match locational and life-style preferences (p. 24). Weiher develops a theory of urban political boundaries that details how boundaries sort populations, as well as the relative sorting effectiveness of different types of boundary configurations. In doing so, he speaks to those working with rational choice theories, locational analyses, and political economy arguments, as well as those concerned more directly with the racial and class segregation prevalent in American society.

Weiher points out that most theories of residential choice processes (e.g., Tiebout's model; the tipping point argument; and Hirschman's exit, voice, and loyalty schema) tend to assume perfect, or at least sufficient, information about places. By failing to account for how people get the information necessary to make choices among places, these extant models cannot explain *both* the exclusionary and the recruitment processes underlying American settlement patterns. To the extent that political boundaries in a race-sensitive culture efficiently provide information about potential neighbors, racial transitions in neighborhoods may occur over time even in the absence of specific discriminatory acts. One consequence of these partial theories of choice processes is possible misdiagnoses of the factors contributing to residential segregation patterns.

Past policy efforts to obviate the effects of residential segregation within cities seem to bear this out. Weiher suggests that initiatives aimed at reducing intrajurisdictional segregation, such as busing and school integration, weakened the salience of informal neighborhood boundaries and gave greater prominence to formal political boundaries. As a result, they restructured residential segregation from the intrajurisdictional to the interjurisdictional level.

Weiher tests whether this shift has occurred and whether variations in boundary configurations are associated with different degrees of residential homogeneity. He compares segregation trends by race and class over time (1960–80) at the neighborhood and municipal levels in Los Angeles and Cook County. His findings indicate that during this time period, geographic segregation by class and race became organized by municipal boundaries, rather than neighborhood borders in both cities (p. 102). In a more detailed study of St. Louis, Weiher demonstrates the information processing values of boundary configurations. Cities with coherent, rather than split, municipal and school district boundaries were hypothesized to have greater spatial segregation by race and class, since the information costs about neighbors and schools are reduced for potential residents. Weiher's analysis of unified and split municipalities in the St. Louis metropolitan area supports this hypothesis: unified municipalities are characterized by greater spatial segregation.

The value of this study is that Weiher takes political fragmentation seriously. He views fragmented political boundaries as a causal factor in racial and class segregation and provides persuasive evidence to support his argument. The analytic focus on context-structuring processes and on the political significance of cognitive spatial perceptions is an especially useful contribution. But his insistence on divorcing the conventional institutional approach from his geographic focus is confusing and possibly unnecessary. Although it centers the analysis on the structuring of political space as a factor in segregation patterns (rather than on the search for overt, discrete mechanisms) this very structuring is ultimately a political act. In the St. Louis case study, Weiher wrestles with this chicken-and-egg issue; in this instance, he shows that the district boundaries were created before major population settlement and remain relatively unchanged. Yet boundary changes of every sort are a more typical, pervasive feature of urban areas. While Weiher's contention that we cannot fully determine the intentions and motives underlying original boundary demarcations (p. 162) is correct, his research demonstrates the pernicious effects of recent boundary changes and the manifest need for explicitly political analyses of these contemporary boundary change decisions.

Although this study is sufficiently compelling to be of interest to a broad audience, there are a series of missed opportunities in Weiher's account. The conclusion is curiously amiss in not addressing the obvious policy implications, namely, what to do in the face of this evidence that political boundaries exacerbate tendencies to urban apartheid. As Weiher argues so convincingly in his analyses, these sorting dynamics include both exclusionary and recruitment processes with significant social consequences. In the 1990s, this means the recruitment of better-off whites and African American choice *makers* into separate, relatively homogenous, suburban areas and the growing isolation of the poor, the choice *takers*, in central city neighborhoods. Rather than embracing these issues, Weiher's conclusion digresses into a discussion of "suburban theme parks" and a brief critique of American local government creation processes as acts of "anti-government," of "escaping the social contract" by imposing the most terrible costs on those with the least resources (p. 182). Finally, by relying on dated analyses of European settlement patterns, Weiher overstates the American exceptionalism argument and overlooks the opportunities for comparative analyses. European cities increasingly exhibit tendencies toward residential segregation by class and multiethnicity at the neighborhood level. These trends merit comment and invite the neighborhood- and metropolitan-level analyses conducted so effectively by Weiher for American cities.

University of Colorado, Boulder SUSAN E. CLARKE

Politics and Jobs: The Boundaries of Employment Policy in the United States. By Margaret Weir. Princeton: Princeton University Press, 1992. 238p. $29.95.

In this impressive study, Margaret Weir explains why employment policy in the United States has followed a distinctive and stunted course of development from the New Deal to the Bush administration. Her approach is that of a policy historian, weaving in-depth research on specific proposals and policies with observations about broader trends, events, and institutional developments to give the reader a clear—yet broad-gauged—understanding of the politics of employment policy.

Why has the U.S. government been so much less involved in labor market planning, worker training and retraining, and job creation than most other Western European governments? The answer, according to Weir, is not simply that a highly developed welfare state would have run counter to dominant American values; that business interests have wielded unusual political power; that racial divisions have infected partisan competition in a way that has limited the possibilities for labor market programs; or that federalism, the separation of powers, and Congress's vulnerability to special interests have led to fragmented and poorly implemented programs. American "exceptionalism" is a product of all these factors and more, especially the power of certain ideas and the nature of certain events.

A central element in Weir's explanation is that decisions made at one point in time establish "boundaries" for policy, which then limit the options available to future policymakers. Her initial analytical endeavor is to explain the failure of "social Keynesianism" (programs designed to ensure full employment through government spending) to take hold in the postwar period by examining the debates leading up to the Full Employment Act of 1946. On its journey to passage this legislation was gutted for several interconnected reasons: business groups (fearing a strong full employment policy would strengthen organized labor) and Southern agricultural interests (seeing a threat to the caste system) joined together to encourage congressional opposition; many members of Congress were wary of the additional power that ensuring full employment would give to the president; there was no federal agency with an established track record in full-employment programming to promote the policy; the attention of presidents (first Roosevelt, then Truman) was absorbed by world affairs; and the economy was performing unexpectedly well.

Weir goes on to argue that after the defeat of social Keynesianism, most federal policymakers and members of the economics profession accepted the view that the level of aggregate employment was mainly a matter of macroeconomic management and that unemployment was largely an individual or human capital problem (thus establishing an important boundary for this area of policy). Still, pockets of poverty in the prosperous United States stimulated some labor market interventions in the early 1960s (area redevelopment, retraining of skilled workers). Then came the War on Poverty, urban riots, black political activism, the distrust of reformers of existing agencies, the proliferation and fragmentation of programs, the politics of race, and the political and perceptual fusion of labor market programs and welfare. Programmatic reform in the guise of the Comprehensive Employment and Training Act was confused and eventually crippled by the rapid and sloppy expansion of public service employment, which added corruption to the already-long list of political liabilities associated with labor market programs. This history, together with the emergence of a new consensus about the need for fiscal austerity and the Reagan administration, led to a dramatic curtailing of labor market interventions in the 1980s.

Weir's historical approach to policy analysis works in many ways. Her examination of policy sequences leads her to put together an explanatory framework that incorporates concepts and perspectives from studies that emphasize the importance of cultural values and public opinion, interest groups and power, ideas and policy innovation, and institutions and processes. The influence of scholars such as Katznelson, Krasner, and Skocpol is apparent, as Weir acknowledges. In the manner of a historian, Weir has delved into the archives of the Department of Labor to come up with some revealing documents. The analysis is also genuinely interdisciplinary in that the research findings and viewpoints of economists, political scientists, and sociologists are included in her discussions and explanations.

It is impossible for a book of this length to explain 50 years of developments in the vast field of employment policy in a way that devotes sufficient attention to all the programs and policies some would prefer to see covered. Weir directs most of her analytical attention to the Full Employment Act of 1946, public service employment under the Comprehensive Employment and Training Act, and the Humphrey–Hawkins Act of 1978. The multitude of programs for youth (Neighborhood Youth Corps, Job Corps, youth initiatives of the Carter administration, vocational education) and the adult training programs (from the Manpower Development and Training Act of 1962 to the Job Training Partnership Act of 1982) receive only cursory treatment. Various private sector initiatives and efforts to link employment and community development are ignored altogether. This selective attention is not a critical flaw, because Weir does manage to work the life cycles of many of these programs into her explanations; and those that are left out would have fit nicely within her framework if they had been included.

Weir is never altogether explicit about her employment policy preferences. At the end of the book, she lays out a very brief sketch of "promising possibilities for assembling new packages of policies" emphasizing community-based initiatives, links to the private sector, and apprenticeship programs (p. 178). One has the sense, however, that she regards the decision not to follow a policy of full employment, with the government serving as an employer of last resort, to have been a major mistake. Gary Mucciaroni's *Political Failure of Employment Policy, 1945–1982* (1990) is very similar to Weir's book. He is much clearer about evaluation of U.S. employment policies and somewhat more complete in his coverage of programs. Both books make important contributions to our understanding of politics and public policy in the United States.

Smith College DONALD C. BAUMER

God's Warriors: The Christian Right in Twentieth-Century America. By Clyde Wilcox. Baltimore: The Johns Hopkins University Press, 1991. 249p. $34.00.

The rise of the Christian Right in the 1980s agitated not only American politics but American political science, as well. Interest in the movement provided an enormous stimulus to the growth of a religion-and-politics subfield, directing attention to the political impact of religious beliefs and institutions. One focus of the new subdiscipline was the Christian Right itself. Although the original movement organizations are mostly defunct, scholars are still producing analyses: some purport to perform postmortems, others claim to trace the Right's transmigration into new forms.

God's Warriors does some of both. Clyde Wilcox draws on a decade of research on the Christian Right, using American National Election Study data and various special purpose surveys. The book commences with an indispensable review of social science theories on the Right's origins, which leaves the reader impressed with the resilience of some rather dubious theoretical perspectives in the face of repeated empirical disconfirmation. This is most notably true of ever-popular claims that the Christian Right, like earlier rightist movements, grew out of status anxiety, political alienation, or authoritarian personalities.

Wilcox counters that the Right has clear religious and demographic roots but that its emergence is best explained as a rational political response of people whose values and beliefs are sharply challenged by modern political, educational, and social transformations. And although the data sources are not really conducive to a final test of the alternative theories, Wilcox's evidence and analysis are quite persuasive.

Many of the author's findings will be familiar to those conversant with his earlier work and the comparable efforts of other scholars; but the book is a very useful and refined summary of research on the popular and activist base for the Christian Right. Wilcox finds that the Moral Majority's supporters consisted of evangelicals and fundamentalists—especially the latter, who were considerably more conservative than other citizens on social issues and moral values; more conservative on economics despite their modest status; and more anticommunist, patriotic, and supportive of defense spending. These ideological perspectives were even more characteristic of activists in the Christian Right.

Wilcox also delineates the internal cleavages within the traditionalist alliance. He shows that the Moral Majority, Pat Robertson, and the prolife movement drew on fundamentalists, charismatics, and conservative Catholics, respectively—three distinctly different Christian Right factions. Such religious divisions have presented a major barrier to full political mobilization and effective action by traditionalist Christians.

On the whole, Wilcox handles the data with sophistication and care. Specialists will want to quibble with his inclusion of Catholics among evangelical Christians if they meet the doctrinal criteria and his frequent use of very complex models with quite modest numbers of respondents. But the book's only major weaknesses appear when Wilcox attempts too much historical analysis with too little data. For example, his argument that there are threads of continuity in Christian Right politics since the 1920s antievolution campaigns is quite problematic; and his comparison of the Christian Anti-Communist Crusade of the 1950s and the John Birch Society with the 1980s movement is especially troublesome. Although the National Election Study had a thermometer rating of the Crusade in 1964, only 25% of the sample claimed familiarity. One suspects that even these voters were responding more to evocative symbols in the Crusade's name than to the relatively obscure organization itself. Another rather dubious and persistent theme is the search for potential Christian Right support among African Americans. All in all, the quest for historical and cross-ethnic linkages is something of a distraction.

Wilcox concludes, quite plausibly, that the Christian Right is probably gone, at least in its 1980s form. He concedes that these organizations never succeeded in mobilizing the potential Christian Right constituency (perhaps because of their extremism) but argues that this potential was limited in any case by both religious and political differences within traditionalist religious communities. Although the Right may reappear in other forms and under new religious entrepreneurs, the incentives for secular politicians to encourage organizational development are gone, since evangelical Christians have become an active core element in the Republican party, thereby obviating the need for special mechanisms to mobilize and convert them.

How firm that attachment is may be determined by the 1992 presidential election, as two Democratic Southern Baptists try to pry their evangelical and fundamentalist brethren away from the GOP coalition. Only then will the story of the early Christian Right mobilization be complete. In the meantime, *God's Warriors* provides the best assessment available of the Christian Right's mass base and electoral appeal.

Furman University JAMES L. GUTH

On Character. By James Q. Wilson. Washington: American Enterprise Institute, 1991. 211p. $24.95.

What could be more fundamental to life than human character? We hope that we and the people we deal with are of good character. Even criminals have been known to lament the absence of orderly (hence predictable) and decent (hence nonthreatening) social life around which to organize their activities. No rules, no rule breaking. Though it is not everything, good character is what we hope for in our children and our friends, in our wives, husbands, and partners. In its form as the moral virtue of the citizenry, Wilson notes, character has been considered the indispensible ground of self-government, without which other desirable features, such as the separation of powers and bills of rights or wise leadership, would be of no avail. Yet it is rarely discussed, perhaps because it seems too elusive, or perhaps, when we ask government to get involved, too coercive, or when we mourn its absence, too threatening. This welcome collection of essays should be read by all political scientists, because it deals with what is most important in our personal and political lives. Wilson defines character serviceably: "To have a good character means at least two things: empathy and self-control. Empathy refers to a willingness to take importantly into account the rights, needs, and feelings of others. Self-control refers to a willingness to take importantly into account the more distant consequences of present actions; to be in short somewhat future oriented rather than wholly present oriented." It is not only the horrors of Somalia and Bosnia that reveal little empathy and less self-control. Alas, there are plenty of American examples: murder, rape, arson, robbery, illegitimacy, muggings, drug abuse, gangs, and so on. Beyond the obvious, Wilson goes on to bring consideration of character into such unlikely places as deficits: "Public finance," he observes, following James Buchanan, "was once subject to a moral constraint, namely, that it was right to pay as you go." Though *character* is not a word often used in analyzing police work, Wilson shows us that its historical origins lie in enforcing "right standards of conduct in public places," rather than catching criminals. Those who think of loitering, drinking, shouting, soliciting, and acting crazy as "victimless crimes" do their communities a disservice by making residents feel bad and unsafe. That function—protecting the character of neighborhoods—explains why Wilson favors foot patrols even if police do not reduce the crime rate. In his own way, Wilson is trying to raise our consciousness so that it is all right for sobersides like us to make moral judgments. "Drug use is wrong," he tells his readers in plain words, "because it is immoral and it is immoral because it enslaves the mind and destroys the soul". *On Character* is a liberating book.

But for whom? Several times Wilson states that interest in improving character is not ideological. He is "surprised to learn that a concern for character is taken as the infallible sign of a conservative disposition." And he speaks favorably of "the growing awareness that a variety of public problems can only be understood—and perhaps addressed—if they are seen as arising out of a defect of character formation." In the sense that every way of life and its rationalizations require certain standards of personal behavior, like honesty and responsibility for one's commitments, this is undoubtedly true. Yet it would not be accurate to say that all of us agree on what constitutes character in action. If equality of condition is the guiding norm, for instance, and its adherents believe that human beings are born good but are corrupted by evil institutions, they are likely to judge that an inegalitarian system, not the errant individual, is to blame. Then the standards that Wilson would wish enforced—like adherence to social values—will be judged oppressive. The person of good character would (pace Rawls) be the one who exposed these false demands and, better still, sought radical change in institutions that would, by reducing inequality, help those who are worst-off. What is perceived to be criminal behavior, such as stealing, might differ if one thought, with Rousseau, that society originated in duplicity and exploitation. Just as each culture constructs its own definition of what human nature is really like and seeks to have it accepted as the only natural way, so what constitutes good character is socially negotiated—which is why, when it is disputed, we become so uncomfortable.

Although social science does "not know how character is formed in any scientifically rigorous sense," Wilson believes, rightly I think, that rigorous study of how different children grow up in America and how they do or do not become "habituated to temperate and moderate behavior" is important not only to understanding politics and public policy but, indeed, to self-understanding. He is prescient in warning that too many subjects are now forbidden to inquiry. Among these are *genetic* factors in character formation, about which he writes an illuminating chapter. If, as Wilson suggests, not only may parents harm children, but children may behave in such an off-putting and unloving manner as to harm their parents (to choose but one of a number of well-validated and subversive examples), there is a lot for us to learn from this well-crafted and persuasive book—not so much in any answers as in the questions it raises.

University of California, Berkeley AARON WILDAVSKY

COMPARATIVE POLITICS

The Territorial Imperative: Pluralism, Corporatism, and Economic Crisis. By Jeffrey J. Anderson. New York: Cambridge University Press, 1992. 252p. $49.95.

The relatively recent phenomenon of sharply different patterns of structural and political adjustment within subnational units of advanced industrialized states is nowhere better studied than in Jeffrey Anderson's recent work, *The Territorial Imperative.* The author examines two declining regions within each of two Western European nations (Britain and Germany) in order to analyze and understand the politics of adjustment within these regions. Rather than using a descriptive model of similar regions in decline, Anderson asks whether the institutional structure—specifically, the constitutional order of each country—is the determining factor explaining different responses in the various regions.

In the choice of two regions within each country, Anderson chose one pair that he described as "monostructural" with a history of both heavy industry and long periods of decline (the British North East and the German Saarland), and one pair that he described as more diversified, with a manufacturing base and a comparatively recent history of decline (the British West Midlands and the German North Rhine–Westphalia). The purpose of such a division was to determine whether a "partisan logic" or a "government logic" would prevail. By this, Anderson hoped to discern whether political choices surrounding structural decline would be shaped by electoral/coalitional considerations or considerations from "the political–institutional characteristics of the region, specifically in its administrative attributes and its constitutional position in the broader policy" (p. 6). Phrased differently, he wanted to determine whether "politics mattered."

In fact, they do matter, along several important dimensions. First, Anderson found that in the declining regions, pluralist responses tended to dominate over corporatist responses. Despite the development of various forms of cooperation among private and public sector actors in each of the four regions, mesocorporatism was weakly developed even in the more typically corporatist regions of Germany. Anderson explains this anomaly by suggesting that regional, mesocorporatist patterns were undercut by "immutable structural factors . . . that rule out the corporatist variant . . . consist of an explicit lack of intent to generate corporatist relations on the part of central officials and, more important, an absence of organizational capability among the actors that inhabit the regional level" (p. 189). While the absence of corporatism is not surprising to observers of British political economy, its absence in Germany is more puzzling. Yet Anderson finds: "German regional policy-making is a highly institutionalized and multi-lateral *intergovernmental* arrangement, but it is not corporatist. Much more significantly, the constellation and capabilities of actors at the regional level conspire to render territorial expressions of corporatism inoperable" (p. 189).

Anderson's explanation for this apparent anomaly fall in the area of what he calls "government logic and the territorial imperative." He argues that "in both countries, the territorial distribution of power creates differences in the interests and capabilities of actors, which affect the implementation of national regional policy, the hierarchy of actors in the regions, the strategies adopted by national and subnational actors, and ultimately the dominant patterns of interaction that emerge" (p. 196). Yet the variations between Britain and Germany have significantly different manifestations.

In Britain, "regional policy is conducted on a centralized basis, such that the decisions about the gains and losses of assisted areas are insulated from the combined pressures of regional interests" (p. 197). Anderson suggests that the absence of an effective middle tier of government produces an "institutional vacuum at the regional level" that is filled by the national government and undercut by parochialism (p. 200).

In Germany, on the other hand, "the multilateral bargaining arena . . . encourages the formation of coalitions among the various Länder and the federal government, each of which is represented at the bargaining table" (p. 197). In this importance sense, "marble cake federalism" (as opposed to the more American-style "layer cake federalism") allows and encourages overlapping and supportive coalitions that can work for common sets of policies. This produces three significant differences from Britain: (1) regional economic problems are explicitly political issues, (2) they are treated as distributive issues within each German state, and (3) the Länder depend much more on their own resources and much less on those of the central government than is the case in Britain (pp. 204–5).

Anderson's model and explanation (esp. of the interaction among levels of government and private sector actors, as well as the absence of corporatism) are compelling for the declining cases he studies. It would be important, however, to test whether his model of regional politics would hold up when nondeclining regions were added to the mix. In the German case, the work of scholars such as Gary Herrigel (on Baden–Württemberg) and Aline Kuntz (on Bavaria) suggest that mesocorporatist models might produce different outcomes. This distinction is important. When a national government is trying to encourage and support regional adjustment, the more successful nondeclining regions would quite likely prove crucial in providing a "cushion" for the national government while the declining regions underwent restructuring. In future work, Anderson should be encouraged to pursue his "territorial imperative" in regions other than declining ones.

University of Georgia CHRISTOPHER S. ALLEN

True Patriot Love: The Politics of Canadian Nationalism. By Sylvia B. Bashevkin. New York: Oxford University Press, 1991. 196p. $14.95 paper.

Charter versus Federalism: The Dilemmas of Constitutional Reform. By Alan C. Cairns. Montreal: McGill–Queen's University Press, 1992. 150p. $34.95 cloth, $12.95 paper.

The Collapse of Canada? Edited by R. Kent Weaver. Washington: Brookings Institution, 1992. 186p. $28.95 cloth, $11.95 paper.

Two of the three books under review deal with Canada's lengthy constitutional crisis. The other deals with one of the underlying causes, Quebec ethnonationalism, but only briefly.

True Patriot Love is a pastiche; several of its chapters are thematically connected, but others more obviously betray their origins as journal articles. It begins with an analysis of the response by several Canadian academics to the 1985 Macdonald Royal Commission's major recommendation that Canada pursue a free-trade agreement with the United States. Sylvia Bashevkin calls her subjects Pan-Canadian nationalists, though the term is surely disputable. According to Bashevkin, Pan-Canadian nationalism consists of public policies undertaken by Ottawa with the intention of limiting U.S. influences and thereby fostering a sense of Canadian national independence and unity. Unanswered—indeed unraised—questions abound: Can policies by the federal government achieve such objectives? Is the United States (and not, say, modern technology) the great threat to national independence? Is Canadian unity possible?

Her analysis employs the prepolitical or sociological categories *in-group* and *out-group*, a "bureaucratic state perspective," and a combination of the two that she dubs an "integrative politics approach." There are many diagrams and charts and even an equation. No doubt, such a methodological gavotte is thought to be required; but readers interested in her commonsense accounts of two "nationalist" policy initiatives should not immediately be put off. Her discussion of Bill C-58, the so-called *Time–Reader's Digest* Bill, and the National Energy Program (NEP), as well as the "antinationalist" Free Trade Agreement, have undoubted merits as public policy analyses.

The in-group-out-group approach, Bashevkin admits, is not very useful when it comes to distinguishing Canadians and Americans. Moreover, the Canadian in-group is not a coherent body, since many of its interests are apparently held in common with the out-group. By bringing to light the implications and limitations of this approach, Bashevkin's argument has the merit of highlighting the most important dimension of Ontario-centered Canadian nationalism, namely, that the out-group responsible in large measure for sustaining the in-group is the United States. In other words, much of what passes in Canada for nationalism is simply anti-Americanism. On the other hand, the common interests between some in-group members and the United States highlights the importance of regionalism in Canada at the same time that it undermines the ability of Ontario-centered nationalists to be more than voices of Ontario regionalism.

Opposition to Bill C-58, for example, was based on the questionable premise that American broadcasts and American periodicals posed a threat to Canadian culture, a term left undefined beyond a nodding reference to Ontario novelist, Margaret Atwood. Her discussion of the NEP Program, which proceeds without the benefit of the in-group-out-group model, illustrates very well (though inadvertently) the importance of central Canadian regionalism sailing under the false colors of Canadian nationalism. Bashevkin presents the NEP as a successor to the nineteenth-century National Policy, an economic and "nation-building" program that enabled economic rents from western agricultural resource production to be captured by central Canada. Likewise, the NEP was little more than a forced transfer of western Canadian resource revenue (this time from oil and gas, not wheat) to central Canada. In each case, the ostensible purposes looked "national" enough to central Canadians and exploitative to westerners. Unfortunately, no evidence of the size of the net fiscal transfer from the western resource producers to Ottawa is mentioned; nor does Bashevkin say what Ottawa did with the money. An in-group-out-group analysis would have been quite instructive here.

Bashevkin is well aware that Canadian nationalism is more or less an Ontario phenomenon and that it is therefore powerfully opposed by "the reality of regionalism," but she does not see this as a fatal objection. Her attempt, for example, to reconcile the more genuine nationalism of Quebec with the pseudonationalism of Ontario intellectuals is, to put it kindly, a failure. There is, quite simply, a nonrecognition of the ethnonationalist reality of Quebec politics. But even leaving Quebec aside, Bashevkin clearly does not appreciate that calls for "national unity" are widely viewed beyond the borders of Ontario as suspect because such slogans have been translated into policies that serve the interests of Ontario above all and because Canadians outside Ontario have noticed.

The somewhat alarming title of the book edited by Weaver, *The Collapse of Canada?* focuses directly on the most important issue of contemporary Canadian politics: the position of Quebec in the regime. The book resulted from a 1991 Brookings Institution seminar that tried to provide answers to several questions. Why did Canada need to revise its constitution? How did the process work? Why has it been so acrimonious? What would happen if Quebec became independent?

The editor's chapter discusses the two major fault-lines in Canadian politics: one between French-speaking and English-speaking Canadians and one between central Canada and "outer" Canada. The geographic areas of division do not entirely overlap; and even conceptually, they must be refined. The first division is not so much between languages spoken as between French-speakers self-identified as ethnonationally Québécois and everyone else, regardless of language. From outside Quebec City to Windsor, Ontario, the "St. Lawrence–Great Lakes corridor," the conflict is chiefly one of political and economic interest between the centers of early (and now often obsolete) industrialization and either poor and nonindustrial or rich and resource-based areas. Weaver provides a sound introduction to how existing political institutions have exacerbated these difficulties.

Stéphane Dion provides a political analysis of what he considers to be the chief factors sustaining Quebec nationalism and its most important political expression, separatism: fear, born of linguistic insecurity, and confidence, stemming from a belief that Quebec can remain prosperous as an independent state. He also mentioned a relatively short-term resentment of "English Canada" (a term meaningless outside Quebec but used among French Quebeckers to refer to all other Canadians) for resisting Quebec's nationalist aspirations. However, Dion ignores the most obvious short-term development, the delegitimization of elite accommodation, as well as a longer-term—and perhaps more important—considerations emphasized by other students of Quebec ethnonationalism, namely, the political consequences of the very rapid secularization of French Quebec society, the transfer of religious sensibilities and enthusiasms into politics, and the great strain this development has put upon the constitutional regime.

Andrew Stark discussed several of the impediments to agreement between Ottawa and the provinces other than Quebec, as well as between Quebec and the other provinces. He centered his argument on four areas of tension: (1) the rejection of special status for one province in favor of former prime minister Trudeau's vision of a bilingual nation; (2) the contradiction between the diffuse left-wing Ontario-centered nationalism, about which Bashevkin writes, and Quebec; (3) the populist agenda of the new Reform party of Canada, supported chiefly in the West, which has no use either for special status for Quebec or for official bilingualism, believing, instead, in equality of provinces and citizens and in the social reality that language is territorial (Stark, along with many easterners, identifies this configuration of political principles as "western alienation"); and (4) an evolving set of demands associated with multicultural, feminist, and aboriginal concerns that rely for implementation on favorable judicial interpretations of the Canadian Charter of Rights and Freedoms.

Keith Banting provides a speculative chapter on the likely outcome if Quebec ever decided for independence. It would, of course, entail great difficulties and adjustments, a prospect not too dissimilar from a continuation of the current situation, which is, of course, what makes Canadian politics at the moment interesting—unfortunately for Canadian citizens, perhaps, but a splendid source of data for political scientists.

Readers looking for an incisive and insightful account of the constitutional dimension of Canada's current difficulties will very quickly encounter the work of Alan Cairns. *Charter Versus Federalism* pursues interpretative themes he has developed in other books over the past few years and amounts to a continuation of a long series of superb essays on Canadian politics. This book began as a 1987 lecture series at Queen's University, Kingston, Ontario. It is devoted to an analysis of the purpose of the Canadian constitution and, more especially, to the impact of the Charter on Canadian society, Canadian political discourse, and Canadian public policy.

Cairns's thesis, broadly speaking, is that the traditional divisions of federalism consisting in the division of powers between the two orders of government, which he calls the governments' constitution, have diminished in importance in light of the "citizens' constitution," embodied chiefly in the Charter. New divisions (gender, ethnicity, disability, etc.), along with vocal citizen support groups, have introduced a novel, nonparliamentary, and nonfederal language into the center of constitutional discussion. However, at the same time that the hitherto centrality of governments in Canadian constitutional discussions has been displaced by a new concern among citizens for the rights of citizens, governments remain the sole agents by which constitutional change may be undertaken. In this conflict between the citizens' and the governments' constitutions lies an important source of Canada's current problems.

Cairns begins by situating the Charter in the context of international legal norms and instruments (many of which originated in the United States). He goes on to discuss general problems of constitution-making within the fragmented Canadian political community—if, indeed, *community* is the proper term to refer to a people divided by conflicts over political principles and possessing highly disparate visions of their country. Cairns ends by applying his distinction between the governments' constitution (in control of the amending procedures) and the citizens' constitution (with its power, indispensable in a democracy, of conveying legitimacy upon governments) to the ill-fated Meech Lake Accord. His analysis of the 1990 failure to achieve a constitutional agreement acceptable to Ottawa, Quebec, the other provinces, and major nongovernmental citizen groups is a model of scholarship. Indeed, Cairns is one of the few commentators on the Canadian constitutional problem who has a remotely clear view of what a constitution—any constitution—is. His analytical remarks are thus well grounded in something approaching a political philosophy. For this reason, 1 can only regret that he approached the topic of Canada's political disorder *sine ira et studio.*

Still, Cairns has provided a splendid corrective to so much Quebec-centered commentary by reminding his readers that the other great constitutional document, the Charter of Rights and Freedoms, has not disappeared below the horizon and will indeed remain whether Quebec remains within the Constitution. One may go so far as to connect the two constituent elements of the constitutional problem with the observation that Quebec's apparent rejection of the equality provisions of the Charter (especially the doctrine that citizen equality takes priority over the Quebec government's self-understanding that its chief obligation is to shelter Quebec's "distinct society") is at the heart of many non-French Quebeckers rejection of Quebec.

University of Calgary BARRY COOPER

The Profession of Government Minister in Western Europe. Edited by Jean Blondel and Jean-Louis Thiébault. New York: St. Martin's, 1991. 215p. $59.95.

Executive Leadership in Anglo-American Systems. Edited by Colin Campbell, S. J. and Margaret Jane Wyszomirski. Pittsburgh: University of Pittsburgh Press, 1991. 416p. $39.95.

In many of the Western democracies, including Italy, France, Germany, and the United States, these are seasons of discontent. Restive electorates testify to a syndrome that has two aspects: domestic problems that seem chronic if not altogether intractable and a global environment in which the euphoria that followed the unanticipated demise of communism has succumbed to uncertainty and unease. Even so widely heralded a change as increasing (West) European unity has been found wanting in light of the region's ineffectual response to the crisis in Bosnia and Herzegovina.

The failure of government leaders in Europe, and America in particular, to come to grips with an array of what ails us (which is not to say that they have failed altogether) puts a premium on studies that in one or another way illuminate leadership roles, institutions, systems, arrangements, patterns, and cadres. In this sense, these two edited collections are welcome additions to a literature that some of us refer to as leadership studies (a subfield still puny by any standard). By and large, they are what their titles suggest: straightforward descriptions of certain aspects of executive leadership in what we generally refer to as democratic systems.

Along with Jean-Louis Thiébault, Jean Blondel expands his already considerable body of work on leadership. This particular collection is reminiscent of a genre that was once a staple of political science—elite studies. Specifically, it is a collective effort aimed at underscoring the similarities and differences among the various models of cabinet government in Western Europe by focusing on ministers. Broadly, the chapters fall into two categories: those that focus on the ministers' personal characteristics (e.g., age, gender, education, occupation) and those that focus on their career characteristics (e.g., duration, continuity, mobility).

The quality of the chapters in this volume is generally high, and the discussion is often buttressed by quantitative data. Having said this, let it be noted that there is nothing here that in any way astonishes. Indeed, most of what proves to be fact would have been presumed anyway. For example, cross-national variations notwithstanding, overall women constitute only 6.2% of all Western European cabinet ministers; these ministers are on average 48-years-old at the time of their first appointment; 77% of them have had some higher education; and generally about half are drawn from three occupational groups: law, teaching, and civil service.

Those familiar with the analogous literature on the United States will not be surprised to read that there are two broad types of ministers in Western Europe: the politician–generalist and the manager–specialist. In his conclusion, Maurizio Cotta writes that a cabinet characterized by ministers with a stronger political background will "lean more in the direction of the representative and political pole," whereas a cabinet where nonpolitical ministers are a significant group "will lean relatively more in the direction of the administrative and managerial pole" (p. 195). Sound familiar?

If not, for further reinforcement, you might try turning to the collection edited by Colin Campbell and Margaret Jane Wyszomirski, which grew out of a conference on executive leadership at Georgetown University. Their introductory chapter, which in fact provides a sturdy summary of the proceedings, concludes that "neither the most party-political appointee nor the most technically competent career official adequately serves the political executive if s/he fails to achieve a creative mix between responsiveness to political exigencies and the rigorous assessment of the effectiveness of policies and programs" (p. 29).

Once again, most of the essays, which were penned by a lively mix of scholars and practitioners, cover ground that is familiar. Staffing and advisory systems are given the most attention; individual executives are omitted from the discussion almost entirely.

The interesting attempt to juxtapose past periods of executive expansion with "the current minimalist era" is perhaps most successfully realized in "Executive Leadership in an Age of Overload and Retrenchment" by B. Guy Peters, which goes beyond description to a quite cogent analysis. Peters poses the question that, while only occasionally posed directly in either of these two volumes, nevertheless must undergird any discussion of democratic government in the 1990s: How can governors in Western Europe and the Anglo-American democracies (including the United States, Canada, and Australia) govern during a time of cutbacks and pullbacks? Peters suggests that the answer may lie in a style of leadership more reminiscent of Jimmy Carter than Ronald Reagan: "Executive leadership may have to be more technocratic (and bureaucratic) and less photogenic if it is to be successful, in policy, rather than only political, terms" (p. 79).

The comparative analyses provided by both of these volumes is a useful reminder of differences in executive leadership that characterize even systems that on the surface would appear to be quite similar. In the end, however, these distinctions may be more of an intellectual conceit than anything else. Since their impact on outcomes remains undetermined, we are left to conclude that for all the idiosyncracies that still characterize our nationhoods, our futures will be very much the same.

SmartDecisions BARBARA KELLERMAN

Democracy and Socialism in Africa. Edited by Robin Cohen and Harry Goulbourne. Boulder: Westview, 1991. 272p. $39.50

This collection of 13 original essays evolved from a conference at the University of Warwick sponsored by the *Review of African Political Economy*. The contributors are scholars of African affairs who came together to explore the relationship between socialism and democracy. An introduction begins with a rather rambling thematic statement that serves as a framework for these essays. *Democracy*, it says, is an ambiguous term, contested between the Left and the Right and also within the Left. Ambiguity has facilitated abuse of the term *democracy* so that without democratic organizations of workers, peasants, and other social groups, popular and constitutional government serves class exploitation and abuse of power. Democracy in Africa is undermined by bourgeois, nationalist, and socialist governments that do not permit free elections or tolerate democratic rights. State, legal, and political institutions are needed to secure people's rights. Socialists must take democracy seriously and ensure that it be renewed within workers organizations and popular organizations and in the society at large.

Within this framework, the essays are clustered into two parts, one around issues and debates, the other on case studies. In the first part, Peter Lawrence examines the question of economic democracy, socialism, and the market and argues that the development of the market is decisive in the allocation of resources and therefore integral to the process of democracy and socialism. He identifies features of African economies as a basis of collectivist economic and political democracy and suggests that market socialism is possible where states so structure markets as to ensure democratic participation by producers and consumers. Ken Post takes up the theme of how the organization of civil society conditions the state and democracy, shows that Africa has not experienced liberal democratic regimes, and outlines an innovative basis for socialism, premised on a pluralistic national party system and a socialist bureaucracy characterized by decentralization of policy implementation, elected delegates (rather than professional bureaucrats), and the formation of a counterbody of elected delegates at every level of bureaucratic agency. Bola Dauda begins with "capability for democracy . . . defined in terms of consensus among African élites to produce institutional frameworks for political stability and peaceful change of government" (p. 53). He emphasizes both equality before the law and human rights in the search for a democratic institutional foundation. He shows that in Africa political decentralization led to fragmentation and instability, because ethnic, linguistic, political, and religious groups could undermine democracy. He calls for a more positive understanding of bureaucracy: "To destroy bureaucracy in order to enthrone democracy is certainly not only unrealistic but also undemocratic" (p. 69). Pepe Roberts and Gavin Williams turn to the interests of peasants and women, arguing that these interests have suffered under capitalism and capitalist farmers, as well as socialist states. Socialism, they believe, needs "[to] address the gender relations of production and reproduction and to reconstruct their ideological pretensions and political practices" (p. 92). Finally, Daryl Glaser looks critically at diversity in the Left's discourse on democracy in the South Africa. He affirms that these conceptions of democracy do not assimilate political pluralism and civil liberties in their model of a democratic socialist society. The struggle for socialism under working-class leadership must incorporate these qualities.

The contributors to the second part of this volume focus on specific case studies: J. Oloka-Onyango on grassroots democratic tendencies in the recent experience of Uganda, Jeff Haynes on the effort to bring about democratization following the coup in Ghana in December 1981, M. A. Mohammed Salih on pastoral society and socialism and democracy in the Sudan, Jack Parson on democracy in Botswana, Chris Allen on the decline of women in postcolonial power and participation (with attention to Kenya and Zambia), and Carol B. Thompson on democracy in a regional African context. In all these cases, democracy is problematic. Particular experiences are identified and analyzed. The prospects for democracy and socialism are not particularly optimistic in Africa.

In his conclusion, Harry Goulbourne points to a resurgence of politics in postcolonial Africa—in particular, the challenge of ordinary people to the monolithic state institutions that have prevailed in the aftermath of independence. He emphasizes the need for minimal participatory rights as a basis for socialism in Africa and sees multipartyism as an essential ingredient to overcome the constraints on civil and human rights imposed by the single-party states of Africa after independence.

Similar to the other contributors, Goulbourne draws upon intellectuals in the advanced industrial countries in order to build his position. Throughout the volume, the theoretical basis of the discussion is for the most part related to European and North American, rather than Third World, thinkers, although the substantive basis relies upon the African experience. There is mention of Nyerere and Nkrumah; Fanon is identified, although references to him do not appear in the index; and Amílcar Cabral is ignored altogether. This contradistinction probably emanates from emphasis on ideal forms of liberalism and socialism, as well as the blurring of representative and participatory forms of democracy. A clearer and sharper delineation of the meaning of formal and indirect democracy, on the one hand, and of informal and participatory democracy, on the other, might lead beyond this exploratory probe into a more comprehensive theory of democratic socialism. As a whole, the volume is a contribution to knowledge of an Africa that is not well understood, a pioneering effort that should serve as a foundation for more elaborate theoretical and substantive understanding of the area's complex politics.

University of California,
Riverside
RONALD H. CHILCOTE

Ethnicity, Integration, and the Military. Edited by Henry Dietz, Jerrold Elkin, and Maurice Roumani. Boulder: Westview, 1991. 227p. $27.50 paper.

Democracy vs. National Security: Civil–Military Relations in Latin America. By Paul Zagorski. Boulder: Lynne Rienner, 1992. 216p. $35.00 cloth, $16.95 paper.

Ethnicity, Integration, and the Military explores the issue of whether or not the military has been, or could be, a force for ethnic integration. Because the armed forces, particularly within Third World states, are presumed to have held influence over both state and society far beyond their traditional roles as defenders of territorial sovereignty, the issue of ethnicity is fair game. Although this is certainly not the first book on the subject, it is one of the few comparative studies. The editors have assembled interesting case studies from eight nations whose political and social differences are quite apparent. Yet despite these differences, the empirical patterns that emerge indicate points of undesired convergence.

Specifically, the authors raise the curtain on the military–ethnic issue by posing three interrelated questions:

1. Has the military socialized its own recruits to accept and practice ethnic integration?
2. Has the military served as a vehicle for civilian integration?
3. Has the military simply mirrored preexisting societal cleavages?

Regarding question 1, the evidence indicates very limited success. Only in the United States has the military clearly stepped ahead of society in its efforts to desegregate white and black recruits. In some cases (Nigeria), the military demonstrated an intent to achieve integration only to find ethnic loyalty rearing its ugly head to interfere. In other cases, exploited ethnic minorities (China) and majorities (India) were integrated into the services but at subordinate positions. And in Israel, the Israeli Defense Force did not so much integrate as assimilate the Oriental Jews in order to preserve the domination of the more powerful Ashkenazim group.

Even where *internal* military integration has, by degree, been achieved, the armed forces have not served as agents of *external* change. In every multiethnic society under review, tensions and divisions between groups were found to persist or worsen. In response to question 2, there is not a single case presented where the military has been a force for *national* integration. Even the all-powerful, ideologically minded socialist states (China, Ethiopia) were unable and unwilling to obliterate ethnic differences. While some success was claimed for the Greek and Turkish cases, such a claim is misguided since these countries were the most ethnically homogeneous of the lot and suffered more from urban–rural, socioeconomic, and ideological cleavages.

Part of the problem is that the contributors often evoked different meanings of integration. Although in some instances, ethnic unification was the goal, in other instances it was economic development, employment, stability, nationalism, or loyalty to the state. Certain armies may have been integrative on one set of criteria but not another. If India's army has, in fact, been the underwriter of that nation's unity and stability by providing a valuable source of employment, that is besides the point of the book. And for all of its success at absorbing recruits from a larger pool of aspirants, the Indian army has not unified its ranks but, at best, better reflected its nation's ethnic composition (question 3). And so it is for many of the other militaries under review. In fact, the goal of ethnic integration seems so out of reach that one wonders whether the authors may have set themselves up for disappointment by having posed the issue in the first place.

The editors could have done a better job at synthesizing the materials. Their framework of three overlapping relationships (military–ethnic, state–ethnic and military–state) could have been useful device. Unfortunately, the concluding discussion criss-crossed over the boundaries of these relations so frequently that the distinctions were thoroughly blurred. On the positive side, one does come away from this book with a greater appreciation of just how intractable the problem of ethnic cleavage is, whether inside or outside of the armed forces. In an age when ethnicity is quickly replacing class and ideology as the world's principal divisive force, this volume is indeed a timely contribution to the literature.

Paul Zagorski's book *Democracy vs. National Security* is an effort to take stock of important political changes that have occurred regarding civil–military relations in five South America countries during the postauthoritarian period. The study is expansive in scope, ranging widely across cases and topics. It is organized thematically, with conceptual chapters followed by issue-oriented chapters devoted to human rights, internal security, military reform, and reform of the state. Based on his "comparative and suggestive," as opposed to definitive, analyses of each of these problem areas, the author then attempts to test a set of hypotheses concerning the conditions under which the armed forces are motivated and capable of resisting civilian initiatives.

The broadly comparative framework of this book is a welcome relief from the single-nation preoccupation of many area specialists. The author has synthesized an impressive amount of information and done so in a way that provides parallel accounts of civil–military affairs in Argentina, Uruguay, Peru, Brazil, and Chile. The comparative design has not been used actually to test assertions. Rather, it has been deployed as a taxonomical device, fitting the data into categories that allow for easy assessments of similarities and differences between the cases.

Like the editors of *Ethnicity, Integration, and the Military*, Zagorski has centered his inquiry on a critical topic, that of civilian control over the armed forces. A key question in the decade ahead will be whether or not governments in Latin America will be able to achieve unconditional military compliance with democratic authority. The author proposes to examine the record of statesmanship up till now to assess how well political elites have managed military affairs in the past and then, based on those findings, extrapolate into the future. But in its attempt to analyze civilian control strategies, the book suffers from faulty conceptualization.

First, strategies suggested along a continuum of possibilities are either unrealistic (wholesale restructuring); recipes for disaster (politicalization); indicators of governmental weakness (client/hostage and coexistence); or end points, rather than strategies (doctrinal change). Second, the author presumes that zero-sum conditions prevail in all but one case. If so, one wonders how a successful control strategy could ever be engineered. Without identifying equilibrium points derived through mutual accomodation (which implies a positive-sum environment), how can governments convince the armed forces that it is in their best interests to cooperate? Ironically, the one area described as positive-sum is precisely where zero-sum conditions are most likely to prevail—doctrinal change. The armed forces are especially defensive when civilians are perceived to be "meddling" in areas of military socialization and indoctrination. Civilian control is unlikely to be achieved in the short and medium term through any direct challenges to the military mind-set. Third, the strategic options are often misapplied to the cases. Though labeled as such, Peruvian president Alain Garcia's reforms did not reflect doctrinal change, at least by the author's account. And the characterization of Peru, Uruguay, and Brazil as instances of coexistence conceals the differences between these cases. Sarney's government in Brazil was probably weak enough to be considered a client of the armed forces, since it remained in power largely through capitulation. Coexistence may have been more applicable in Uruguay and Peru. But then again, the singular label disguises fundamental differences between these two countries, as well.

It is the well-conceived and well-researched book that is able to find support for its central hypotheses. But of the 16 or so hypothesis tests that were run in this book, I count only two or three partial confirmations, and they are weak. Many were disconfirmed, and the rest could not be properly evaluated for lack of evidence. Insufficient care was devoted either to the formulation of these hypotheses or to the systematic gathering of evidence to support them. The author's factual accounts were rich and variegated. Unfortunately, his facts simply would not cooperate with his theories. Not surprisingly, the conclusions are ambivalent, leaving the reader to doubt whether the overall objectives of the book have been achieved. These criticisms notwithstanding, I would recommend this book to scholars and students of Latin American politics, because it contains useful information about a timely and unquestionably important topic.

University of California, Riverside DAVID PION-BERLIN

Formations of Violence: The Narrative of the Body and Political Terror in Northern Ireland. By Allen Feldman. Chicago: University of Chicago Press, 1991. 328p. $48.00 cloth, $17.95 paper.

Formations of Violence is one of the best books to have been written on Northern Ireland, ranking, for example, with Rosemary Harris's *Prejudice and Tolerance in Ulster* (1972). In the broader field of political science, however, the greatest mark of *Formations of Violence* will undoubtedly be its contribution to the study of political violence.

Applying and expanding upon the work of such scholars as Michel Foucault, anthropologist Allen Feldman describes his book as "an ethnography of surfaces—those sites, stages, and templates upon which history is constructed as a cultural object" (p. 2). The events analyzed cover the period from the rioting in 1969 through the Hunger Strike by Republican prisoners in 1981. Oral histories of working-class Protestants and Catholics were obtained during the period 1984–86. The discussion of this delicate process, along with the theoretical implications of interpreting the data, will be of particular interest to field researchers (pp. 10–16).

The extensive selections from the oral histories are instructive and intriguing. After noting the operational and political disadvantages of assassinations at the victim's home, for example, one nationalist paramilitary observes, "It demoralizes the police stiffing them at home. Everybody and particularly a cop wants to come home off the job and relax. . . . The wife is going to demand that he gets out when one of his mates gets whacked. That's the whole purpose of that type of operation" (p. 76).

Feldman criticizes approaches based upon "processual models" of political violence, such as those that focus on economic deprivation, political discrimination, and ideology. In these approaches, "violence is denuded of any intrinsic semantic or causal character" (p. 19). Instead, he emphasizes the importance of analyzing violent interchanges themselves: "Novel subject positions are constructed and construed by violent performances, and this mutation of agency renders formal ideological rationale and prior contextual motivation unstable and even secondary" (p. 20). Feldman's work does much to illuminate the nature and importance of these exchanges. His analysis is insightful and often brilliant, though his conclusions may sometimes be best viewed as interpretive, rather than definitive.

The idea that violence is not merely a dependent variable has merit, but an exclusive focus on violent interchanges would also provide a distorted view of the causes of political violence. *Formations of Violence* treats other key areas essential to a full understanding of the Northern Ireland problem only tangentially. International dimensions, such as the role of the Republic of Ireland and various countries and international organizations, have had a significant impact on the nature of violent activities by both the state and paramilitary forces. It seems clear that such variables as economic conditions and discrimination are also essential in understanding the complex phenomenon of political violence in most settings, including Northern Ireland. Of course, the limited focus of the book is deliberate and enables the author to exam his subject in depth; but a rejection of the importance of other modes of analysis would be unwarranted.

Formations of Violence will be of great interest not only to students of Northern Irish politics but also to scholars interested in all aspects of political violence. Because of its novel dissection of many law enforcement and insurgency practices, it will be of interest to scholars, policy analysts, and practitioners in these areas. Scholars concerned with broader questions of authority, legitimacy, and political culture will find the volume of value. The book is a fine example of poststructuralist research in anthropology and sociology, and it will undoubtedly be regarded as a significant contribution in those areas. For political scientists unfamiliar with this literature, *Formations of Violence* constitutes a useful introduction.

Unfortunately, the prose used in much of the book is ponderous and jargon-laden. Although the volume is unsuitable for most undergraduate political science courses, students would benefit from reading assigned passages at their reserve library. It might be a useful companion text in such graduate courses as those dealing with ethnic conflict and terrorism.

In short, Allen Feldman has written a highly imaginative and significant book. *Formations of Violence* is an important addition to the literature on political violence.

Northeastern University DAVID E. SCHMITT

Constitutions in Crisis: Political Violence and the Rule of Law. By John E. Finn. New York: Oxford University Press, 1991. 274p. $35.00.

The rise and fall of democratic constitutions after World War I stimulated the richest and most fruitful speculation about constitutional crises since the American and French Revolutions. The works of Carl Schmitt, Hans Kelsen, Carl J. Friedrich, Frederick Watkins, and Clinton Rossiter, among others, come to mind, works that contributed to the soundness of the Basic Law of the German Federal Republic (the West German constitution) after World War II. Curiously, despite the enormous interest in the nature of democracy in the second half of the twentieth century, academics contributed little to this fascinating critical and perennial question.

John Finn's thoughtful work reopens the subject; and it will surely stimulate other studies, especially because the end of the Cold War will provide many new cases to ponder. Initially, Finn examines the theoretical literature—the political and legal controversies surrounding the suspension of normal constitutional procedures. He accepts the necessity of crises powers (virtually all democratic constitutions permit them) and offers criteria for assessing the practices. Finally, he applies those criteria to British (Northern Ireland) and the two German experiences, those of the Weimar Republic and the West German Federal Republic.

An appropriate exercise of emergency powers demands a clear understanding that constitutionalism requires us to give reasons publicly. Indeed, in this view, human dignity is inseparable from this commitment to reasoning. For Finn, this means, inter alia, that those vested with emergency powers must be held accountable (i.e., by a separation of powers) and that sometimes we must go beyond the restoration of a particular constitution to reconstruct the social order to make constitutionalism possible.

The detailed discussion of how British regulations since the 1920s have worked is informative and impressive. He finds the record mixed. Some provisions, such as the suspension of trial by jury, satisfy the principles Finn articulates, while others (e.g., measures to permit arbitrary arrest and detention, domestic travel restrictions on citizens, and certain forms of interrogation) do not. Most worrying is a persistent and pernicious tendency to ignore the necessity of making administrations accountable.

No European republic had to use its emergency powers to curb rebel violence as often or in as many ways as Weimar did. At first, they were often used irresponsibly and ineffectively, without adequate supervision by the judiciary; but between 1924 and 1929 the record was more positive. However, Hitler used Article 48 to destroy the republic. Did that prove that it was foolish ever to create such powers? Perhaps, but Finn notes that there was very little judicial control over Article 48; and—what was more decisive—the republic was betrayed by its philosophical confusions. Kelsen's argument that any party that competed successfully in elections should govern prevailed over Schmitt's that only "friends of the constitution" could legitimately exercise its powers. (Ironically, Kelsen was always a bitter foe of the Nazis, while Schmitt later embraced them.)

In any case, those who made the West German constitution believed that the lesson of Weimar was that one should pursue emergency powers more, not less, aggressively, providing facilities for outlawing parties inimical to constitutional principles, establishing an Office for the Protection of the Constitution, and subjecting all actions to careful close judicial scrutiny. To their credit, the Germans have avoided British inclinations to utilize special courts with extraordinary, often unsupervised procedures. But in Finn's detailed discussion of the situation, he notes important instances where the zeal inspired by the Weimar trauma may have gone too far, the most notorious being the questionable procedures used in dismissing large numbers from the civil service for promoting unconstitutional causes. The perceptive analyses of the German cases are persuasive, but one regrets the scarcity of German sources.

Finn studies the problem largely as the American Revolution defined it through *The Federalist Papers*. He does not mention the second major notion of emergency powers, which the French Revolution produced, that of the "revolutionary government" whose task is to create the conditions necessary for democracy. (Superficially, this concept resembles Finn's brief reference to emergency powers to reconstruct the social order for constitutionalism.) But the French notion became the source of the "dictatorship of the proletariat" and the theories of "one-party democracy" and "guided democracy" that have animated most Third World states since World War II. The attempt to implement it failed dismally nearly everywhere, and understanding why would help clarify better the problems that Finn does study.

Constitutions in Crisis is a fair-minded, careful, illuminating, and well-written book that will deeply interest a wide range of political scientists—specialists in constitutional law, political theory, comparative politics, and terrorist studies.

University of California, Los Angeles — David C. Rapoport

Chinese Village, Socialist State. By Edward Friedman, Paul G. Pickowicz, and Mark Selden. New Haven: Yale University Press, 1991. 336p. $35.00.

From Bandwagon to Balance-of-Power Politics: Structural Constraints and Politics in China, 1949–1978. By Avery Goldstein. Stanford: Stanford University Press, 1991. 362p. $39.50.

In *Chinese Village, Socialist State*, three American Sinologists recount in vivid detail the effects of two decades of war, revolution, and communist agrarian policy upon the inhabitants of a single rural county in north China. Spanning the period from Japan's 1937 invasion to the collapse of Mao's ill-conceived Great Leap Forward in 1960, the authors—a political scientist, a historian, and a sociologist—weave their narrative into a virtually seamless tale that is one part local history, one part rural political economy, and one part general systems analysis. While no single rural microcosm can ever adequately capture the full spectral richness of a macrocosm as vast as China, Raoyang County, Hebei Province, comes as close to being a valid synecdoche as one would wish for.

Basing their study on a decade of field research involving dozens of site visits and thousands of hours of local interviews, the authors first explore the question of why the communists triumphed in the rural areas of north China. Though they break little new ground on this subject, their research tends to support earlier studies (by, e.g., Kang Chao and Philip Huang) that found that a long-term decline in rural economic health and welfare worked to the clear advantage of Chinese communist insurgents, as did the rise of peasant nationalism in the face of a brutal Japanese occupation (a thesis first articulated by Chalmers Johnson). Equally significant, in their view, was the Chinese Communist party's (CCP's) enlightened, multiclass program of popular mobilization for patriotic resistance and agrarian reform (a theme previously stressed in the work of Mark Selden and Robert Marks, among others). The net result, they conclude, was a "honeymoon atmosphere" of broad popular support for the new socialist state in the rural areas of north China after 1949.

The honeymoon lasted longer in some places than in others. In many of the so-called early-liberated areas of Raoyang County, where the Party had established firm local roots among the village population well before 1949, peasant support generally remained strong until the mid-1950s. In some of the later-liberated regions, however, the honeymoon ended rather more quickly, as overzealous young urbanites, sent by the Party to the countryside to mobilize poor peasants to vent anger against landlords in the land reform campaign of the early 1950s, often installed "vengeful young toughs full of rage and hate" as new village powerholders, resulting in a series of cruel and divisive personal vendettas.

After 1952, when the state took over the marketing of grain and imposed compulsory low prices for the purchase of agricultural products, China's traditional, highly resilient rural market structure began to atrophy. Peasant entrepreneurship quickly evaporated, while rural labor mobility diminished and underemployment became endemic. In Raoyang County, discontent began to surface around the time Mao Zedong decreed a speedup in agricultural collectivization in the summer of 1955. Thereafter, the delicate rural consensus in support of Communist party leadership and policy began to come unraveled. Voluntary cooperation, based on principles of gradualism, self-interest, and careful preparation of public opinion among the peasant masses—hallmarks of previous CCP rural mobilization drives—went by the boards in the new Maoist campaign to eliminate private wealth and prevent rural class polarization. Although many peasants in Raoyang and elsewhere could appreciate the advantages of pooling resources and dividing labor within small, seasonal cooperative groups, most saw big, impersonal collective units as unworkable. Faced with heavy pressure to conform, however, "no one dared to state such truths at the time" (p. 186). Indeed, anyone resisting instant collectivization could be branded as a "rightist." Thus began a pattern of coerced conformity and dissemblance that ultimately spelled disaster for the CCP's agrarian policies.

Describing in personalized detail how the people of Raoyang County were sucked willy-nilly into the maelstrom of Mao's Great Leap Forward in 1958, the authors present a sociology of crisis that is rich in analytical insight and human pathos. Subject to intense pressures from above to realize immediate gains from the formation of large, impersonal, egalitarian, self-reliant people's communes, rural cadres in 1958 began to coerce peasants to participate in half-baked production schemes, such as the ill-fated campaign to manufacture backyard steel and the equally misguided attempt to raise crop yields by subjecting all available farmland to extreme close planting and deep ploughing. When such measures caused a sharp drop in farm output, anxious local cadres, rather than acknowledging failure, began to falsify production statistics. To placate higher authorities, they gave the impression of unbounded peasant enthusiasm for the new rural communes. Reacting to such news, high-level leaders soon became "dizzy with success" and steadily revised their economic demands and expectations upward.

In the prevailing climate of official unreality, the label of "rightist" awaited anyone who spoke out against the fervid fundamentalism of the day. Consequently, few voices were raised in dissent. Serious famine ensued, followed by an epidemic of petty rural corruption and mismanagement. By 1960, not even Mao could continue to deny the extent of the crisis; and China's leaders were forced to backpedal rapidly. Two years and 20 million deaths later, the Great Leap was just a bitter memory, and its organizational centerpiece, the people's communes, all but dismantled. The CCP has never been quite the same since.

In addition to providing a poignant, personalized narrative history of the rise and decline of agrarian collectivism and communalism in Raoyang County, *Chinese Village, Socialist State* achieves notable success at other levels of analysis, as well. One of the book's signal accomplishments lies in its sophisticated elaboration of the various ways in which Raoyang's inhabitants sought to cope with the economic exigencies and political uncertainties of rural life in the stressful 1950s. Two broad survival strategies were in evidence. For those (relatively few) individuals, families, and villages with links to the "inner circle" of state and local powerholders, self-interest dictated frequent displays of loyalty to political leaders and exaggerated devotion to the Party's fundamentalist ideological norms. For the powerless, excluded majority, survival generally required the construction of elaborate "back-door" strategies and mechanisms of evasion: "Whereas the excluded sought to survive outside the walled-off socialist system, any favored unit was compelled by the structure of the system to try to grab everything. . . . The favored, logically, favored more socialism" (pp. 274–75). In thus laying bare the underlying structural logic of rural politics, the authors clearly reveal how a patriarchal tradition of feudal hierarchy and privilege served to reinforce the most untenable elements of communist fundamentalism, thereby thwarting the rationalizing imperatives of modernization and reform.

One of the book's most telling insights concerns the crucial importance of personal and familial contacts and relationships in the determination of who got what, when, and how in postrevolutionary Raoyang. By carefully recounting local family histories, the authors show how the ebb and flow of family fortunes often hinged upon threads as thin as a well-placed in-law in local government or a chance encounter with a former schoolmate or army colleague. Previous studies, by Lucian Pye and others, have highlighted some of the more salient general characteristics of informal Chinese patronage networks; but none has offered such a vivid, intimate glimpse of the anatomy and operations of this quintessential Chinese institution. This is a gem of a book.

A fundamentally different approach to understanding Chinese politics is assayed in *From Bandwagon to Balance-of-Power Politics*. Instead of utilizing local history to generate insight into the structural logic and operations of the larger political system, author Avery Goldstein turns the process around, using insights derived from general systems theory to explain observed shifts in the dominant patterns of Chinese political behavior.

Elaborating upon theoretical models of bandwagoning and balance-of-power politics developed by his Berkeley mentor, Kenneth Waltz, Goldstein fashions a highly competent and literate—if somewhat formalistic and overdetermined—study of the structural logic of Chinese politics. Following Waltz, he identifies bandwagoning as the observed tendency for a sudden rush of consensual support to build behind an emerging winner or rising hegemon once the outcome of a political contest is no longer in doubt. Balance-of-power politics, in contrast, occurs when a rising hegemon induces formation of a defensive coalition among groups whose interests are threatened by the hegemon's potential domination. Other things being equal, bandwagoning is putatively characteristic of "hierarchically ordered, functionally undifferentiated realms in which power is tightly concentrated in the hands of superordinate actors" (p. 57), while balance-of-power politics is more likely to be encountered in anarchic realms, where "functions are differentiated . . . and political decision-making is distributed among specialized actors" (p. 48).

Applying these ideal-typical constructs to the Chinese communist political system, Goldstein argues that from 1949 to 1966 the People's Republic of China exhibited all the salient characteristics of a bandwagon polity: tight hierarchy, unified central leadership, undifferentiated structures of power, and obsequious subordinates eager to demonstrate their loyalty to superiors. Following the advent of the Cultural Revolution, however, the system underwent a dramatic shift in the direction of a quintessential balance-of-power polity marked by high levels of elite conflict, blurred lines and patterns of political authority, rampant factionalism, and an accompanying attenuation of traditional norms of political competition and conflict.

To support his argument that a fundamental structural alteration of the Chinese polity occurred in the mid-1960s, Goldstein provides a number of case studies of political conflict on both sides of the Cultural Revolution divide. For the pre-1966 bandwagon period, he selects the 1954 purge of Manchurian Party leader Gao Gang, Mao's 1955 decision to accelerate agricultural collectivization, the 1957 "Hundred Flowers" campaign, the 1958 rural communization drive, and the 1959 purge of Defense Minister Peng Dehuai. In each of these celebrated pre–Cultural Revolution episodes, at least some of Mao Zedong's principal lieutenants had initially sided with challengers seeking to alter the political status quo. In each case, however, incipient intraparty dissent and heterodoxy quickly melted away once Chairman Mao gave a clear, unambiguous signal of his personal intentions. Thereafter, in a veritable orgy of political affirmation, all major leaders and factions quickly closed ranks, jumping on the Maoist bandwagon and, where necessary, issuing contrite self-criticisms to dispel lingering doubts of their own political loyalty and reliability.

The basic structure of the political system began to change, Goldstein argues, during the Socialist Education Campaign in the mid-1960s, when Mao sought to promote ideological struggle and political consciousness raising as antidotes to the "modern revisionism" allegedly being peddled by Nikita Khrushchev. Despite the single-minded intensity with which the aging chairman launched his assault on "capitalist tendencies" in China and abroad, Mao lacked a coherent plan of attack. His directives were vague, his speeches abstract and enigmatic. They could not readily be translated into concrete programs and policies, even by such presumably loyal lieutenants as Liu Shaoqi and Deng Xiaoping. By 1965, Goldstein argues, "neither Mao nor anyone else knew concretely how to address the danger of revisionism in China," with the result that "political cues as to the importance of 'class struggle' and 'preventing revisionism' could . . . elicit no obvious practical response from subordinates" (p. 150). Under these circumstances, the traditional bandwagon effect failed to materialize, as even the most ardent Maoists faced a formidable task in trying to understand (let alone satisfy) the Chairman.

In the emerging chaos of the Cultural Revolution, political actors could no longer rely on traditional, unambiguous cues from above to orient their behavior. With factionalism running rampant, multiple power centers emerged to compete for influence and authority. Though Mao continued personally to intervene to set the terms of political and ideological debate, his periodic Olympian exhortations—"Unite!" "Seize power!" "Support the Left!"—were devoid of meaningful behavioral guidelines. Anarchy reigned as political actors at all levels were left to fend for themselves as best they could.

By the time the chaos began to subside in 1968–69, bandwagoning had been supplanted by a balance-of-power polity in which various leadership factions were compelled by the anarchic structure of the system to forge a series of temporary alliances to check powerful rivals and preempt potential hegemons. As examples of the new balance-of-power polity in action, Goldstein recounts the 1971 purge of Defense Minister Lin Biao and the 1976 "second fall" of Deng Xiaoping.

Up to a point, the elemental structural logic of Goldstein's argument works rather nicely. Evidence of bandwagoning and balance-of-power effects can indeed be discerned in his case studies, as presented. Yet there is excessive formalism here, along with a certain amount of overdeterminism.

More than two decades ago, a quasi-totalitarian Mao-in-command model was posited that had many of the same basic operational attributes as Goldstein's bandwagon polity. A powerful, charismatic Mao Zedong, unfettered by institutional checks and balances, elicited exaggerated displays of loyalty from anxious colleagues and subordinates whenever he made up his mind to do something, or purge somebody. By the same token, several studies of the origins of the Cultural Revolution (by Franz Schurman, Frederick Teiwes, and Lowell Dittmer, among others) have documented the rapid decline of traditional norms of intraparty consensus building that occurred in the wake of Mao's disastrous Great Leap Forward. While it may be argued that Goldstein's structural constraints approach is considerably more sophisticated than either the idiosyncratic, quasi-totalitarian Mao-in-command model or the "shattered consensus" model that widely supplanted it, it is by no means clear that there is substantial added value to be gained from substituting the one for the others.

Still, Goldstein's twin models would have much to recommend them, at least heuristically, were it not for a recurrent Procrustean tendency to shoehorn messy, odd-shaped, or complex phenomena into rigid, simplistic structural molds. Although his case studies are generally well selected and skillfully presented, Goldstein tends to give them excessive "spin" in the direction of his preferred structural explanation. For example, in his account of the case of Peng Dehuai, he argues that the sudden closing of ranks that followed Mao's decision to purge Peng was due to the fact that in a hierarchical bandwagon polity, "all actors have an interest in ensuring the vitality of the dominant coalition of which they will be a part" (p. 90). Yet at least some participants in the Peng Dehuai affair (including Peng's successor, Lin Biao) have recalled the incident rather differently, arguing that the bandwagon effect was produced not by any widespread desire to participate in the spoils of victory but by a universal fear among Mao's lieutenants of running afoul of the erratic Chairman's violent, mercurial temper.

By excessively palpating and homogenizing his data to achieve the desired structural effect, Goldstein comes very close to reducing all significant political outcomes in China to the function of a single systemic variable—hierarchy versus anarchy. In so doing, he risks seriously trivializing both his models and the complex political processes they purportedly depict.

University of California, Los Angeles RICHARD BAUM

Health Insurance in Practice: International Variations in Financing, Benefits, and Problems. By William A. Glaser. San Francisco: Josey–Bass, 1991. 542p. $70.00.

Health Insurance in Practice is characteristic of Bill Glaser's considerable contribution to the field of health politics and policy. It is invaluable to anyone fairly new to the subject. In this book, as with his earlier ones on cognate topics (*Health Insurance Bargaining* [1978], *Paying the Doctor* [1980], *Paying the Hospital* [1987]), there is a distinctive combination of illuminating scholarship and off-putting style.

What is annoying may be unavoidable in Glaser's scholarly project. He aims to write definitive tomes on his subjects. This book's subtitle might have been *Everything about Health Insurance You Did Not Even Know To Ask*. Such works cannot help but be weighty (542 pages) and somewhat dry. At the same time, Glaser has long believed that America will eventually adopt something like European health insurance; and he is frustrated by both wrongheaded opposition to social insurance institutions and the persistent American failure to learn from the extensive experience with such institutions elsewhere.

As a result, this book alternates between long passages of minute description with jarring bursts of sweeping condemnation—usually directed at American political leaders or, especially, health economists. Many of the attacks are substantially justified, but too many seem overdone. For example, he notes that Americans "could preserve a competitive economic market in second-tier health insurance . . . [and] American health care economists would still have a market to study" (p. 179).

While Glaser identifies some important differences between American and other political contexts, here too, he tends toward sweeping assertions that overstate both the extent and implications of differences. He makes a strong historical case for statutory health insurance being grounded in notions of social solidarity that are much weaker in the United States. But Glaser also demonstrates the limits to that solidarity (e.g., in the case of farmers and the self-employed) and shows how those have been overcome by largely pragmatic choices. And while it would be surely be easier to create universal coverage from a base of solidarity and compassion (p. 495), countries without that background, like Australia and Canada, have found other ways.

It would be a mistake, however, to underestimate Glaser's contribution, particularly for newcomers to comparative health politics and policy. Without using the term, he illustrates for the political scientist the history and importance of *corporatism* in statutory health insurance throughout Europe, especially in Germany, France, the Netherlands, Belgium, and Switzerland.

Glaser provides an extended guide to the realities of insuring citizens against the unpredictable costs of illness. He underscores the importance of physician involvement in the sensible administration of universal health insurance and avoids the doctor bashing that comes so easily to the academic mind. This work, like earlier tomes on paying doctors and hospitals, explicates the different incentives particular methods of reimbursement entail. And he convincingly shows how services such as dental, mental, and long-term care pose quite different problems for insurance itself that may justify different structures of benefits, regulation, and remuneration.

More than a guidebook, *Health Insurance in Practice* is a wise reminder of fundamental constraints on what nations can do in this costly, contentious field. For example, he is clearly right that once a pay-as-you-go system of social insurance has matured, a country cannot switch to a funded one, because it would financially burden one generation twice: "Modern economies now have difficulty paying for one set of payroll taxes, much less two" (p. 23). On the other hand, he may surprise readers with the acute observation that the United States has already dealt with the hardest part of health insurance redistribution by covering the elderly through Medicare. And he notes all sorts of interesting linkages, such as the German problems with long-term care that arise from the strict separation of pension and medical care funds.

In pursuing his wholesome task, Glaser has organized chapters not by country but by theme and medical service. This necessarily involves some repetition, for example, in the separate discussion of interdependent topics like payroll taxes, hospital care, and the advantages of statutory health insurance. Moreover, organizing each chapter with a "Summary" and "Implications for the United States" adds to the dry repetitiveness, making this book something like a string of General Accounting Office reports, save for the occasional haymaker.

Yet for those interested in designing health institutions, such a structure makes sense. And it has a didactic function as well, namely, to force the reader to think in terms of concrete choices and problems—options in the real world, with observed advantages and disadvantages, supporters, and opponents, instead of deductive propositions. Above all, this is a book not of political science or political economy but public administration. If one comes away from it believing that health administration is serious and complicated, so much the better. Besides, Glaser is wiser than most on the subject.

But he is neither infallible nor restrained in his judgments. "Reforms will be blocked" in America, he tells the reader by all manner of interest group maneuvering (pp. 148–49). "Small business can block the enactment of mandates," he notes (p. 179), in spite of countless counter-examples and the likelihood they will be extended in our current round of reform. He attributes more power to the American Medical Association than is justified, claiming that "no law can be enacted in the United States that supersedes the private health insurance carriers with government agencies" (p. 498), a judgment that may very well be overcome by events before this book is widely read.

Nonetheless, *Health Insurance in Practice* is a welcome antidote to much of the ahistorical and apolitical theorizing about medical care reform that appears in print. Like oatmeal, Glaser's work is good for you, despite the occasional lump of missionary overstatement or numbing erudition.

Yale University THEODORE R. MARMOR

Monetary Sovereignty: The Politics of Central Banking in Western Europe. By John B. Goodman. Ithaca: Cornell University Press, 1992. 239p. $29.95 cloth, $12.95 paper.

The inflationary consequences of monetary policy decisions affect the wealth and well-being of just about every member of a modern industrialized society. Yet the elected representatives of the people have a tendency to give up control over monetary policy to more or less independent central banks, while jealously guarding their prerogative to set taxes, social transfers, tariffs, and quotas. As a consequence, many economists and (perhaps more surprisingly) some political scientists have contributed to the myth, carefully nurtured by central banks themselves, that central bank decision making is isolated from political influences. Due to the supposedly esoteric nature of its subject, monetary policy is thought to be dominated by technocrats whose nonpartisan professionalism and expertise ensures that monetary policy is set in the public interest. The popular notion is that monetary policy has the characteristics of a public good—a view that ignores the distributional effects of inflation across debtors and creditors, the employed and the unemployed, the young and the old.

In the United States, a group of scholars has abandoned this apolitical approach to propose that monetary policy is subject to political business cycles and interest group pressures. Another literature links various features of central bank laws and appointment procedures to the monetary policy performance of central banks in cross-country comparisons. The case studies of three European central banks in *Monetary Sovereignty* synthesize and expand on these literatures. John Goodman analyzes how domestic political factors are channeled through the membrane of political institutions to affect monetary policy outcomes. In particular, he argues that political business cycles and interest group pressures are muted through the institution of central bank independence. This emphasis on the role of institutions fits in nicely with the current interest in questions of institutional design.

Moreover, in contrast to many other analyses of central banking that are implicitly based on a closed-economy assumption, Goodman's book analyzes how central bank independence (and thus monetary policy) changes over time as the international interdependence between countries increases. In recent decades, world capital markets have become more integrated and the members of the European Community are moving toward greater economic and social integration. This angle is especially topical, given the current interest in analyzing the interaction between domestic politics and international relations.

Goodman reconstructs the monetary policymaking processes of three central banks, the Deutsche Bundesbank, the Banque de France, and the Banca d'Italia, for the years 1973–85. He then explores their interaction in the period of heightened international interdependence and European integration of the late 1980s. The choice of central banks is a wise one. The German, French, and Italian economies and societies have much in common; but their central banks are not equally independent. The German central bank is widely considered to be one of the most independent central banks in the world, the French central bank is subservient to its government, and the Italian central bank initially had little autonomy and then gained some independence in the 1981 "divorce" from its government.

Two features of Goodman's work are to be applauded. First, the U.S. scholarship on the politics of central banking tends to center excessively on the U.S. case; that is, scholars tend to overestimate the generality of the conclusions that can be drawn on the basis of an analysis of the Federal Reserve System and its interaction with the president and the Congress. Second, regression analyses that link measures of monetary policy performance and indexes of central bank independence, while valuable, are not sufficiently rich in institutional detail to inform discussions on the design of central bank institutions. Here, Goodman's study provides much-needed depth.

Somewhat weaker is Goodman's review of the economic theory underlying monetary policy. Clearly, an analysis of the relationship between the institutional features of central banks and monetary policy outcomes is interesting only if "money matters," that is, if monetary policy has some effect on real variables such as output or unemployment that people (and thus policymakers) ultimately care about. The rational expectations revolution in economic theory, however, has contributed to our understanding of the limitations policymakers face when manipulating monetary policy instruments to systematically influence real outcomes. It is widely accepted, however, that in the short run, monetary policy can affect real variables in the presence of price rigidities. Overall, the theoretical foundations of why "money matters" are controversial and in flux.

Relative to current developments in economic theory, the review in Goodman's book is somewhat dated. As a practical matter, this flaw is of little importance. Policymakers and central bankers cannot afford to wait until the macroeconomic debate has settled; and they appear to believe in, and operate according to, the models reviewed by Goodman. For example, the Keynesian Mundell–Fleming model that derives the effects of monetary policy in an open economy may have gone out of fashion in academia; but it is certainly one of the standard workhorse models underlying central bank decision making.

More serious, however, is the author's failure to review and utilize the concept of the time consistency problem inherent in monetary policy that is widely considered to provide *the* theoretical underpinning for central bank independence. The time consistency problem can be illustrated for the European context. A policymaker would like to utilize monetary policy instruments to stimulate employment and output. Once nominal wage contracts are set, the policymaker can inflate, thereby reducing real wages and increasing employment and output. Thus, given the nominal wage rigidity, the policymaker can, in principle, use monetary policy to manipulate the real economy. However, at the time of negotiating nominal wage contracts, the wagesetters (trade unions and employer organizations) are aware of the policymaker's future incentive to lower their real wages by inflating. Rational wagesetters will consequently add an inflationary markup into their wage contracts that reflects the expected inflation rate. The policymaker is then forced to accommodate their expected inflation rate to prevent an undesired contraction of employment and output. In equilibrium, the policymaker's *futile* attempt to stimulate the economy leads to an undesired inflationary bias. The time consistency problem makes a distinction between ex ante and ex post preferences over monetary policy. *Ex ante,* the policymaker who could credibly commit to refrain from manipulating the economy would be better off *Ex post,* once nominal wage contracts are set, the policymaker has an incentive to stimulate the economy.

The time consistency problem in monetary policy is mitigated if the policymaker grants independent decision-making powers to a central bank whose officials have preferences for a more restrictive monetary policy. By failing to acknowledge the role of this problem, Goodman ultimately does not provide a convincing motivation for the "paradox" that elected politicians are willing to give up power to a central bank that will implement a more restrictive monetary policy than the one desired by the government.

One important insight developed in Goodman's book is that counter to popular opinion, even a very independent central bank must occasionally accommodate its political principals' demands in order to safeguard its autonomy for the future. Nevertheless, Goodman's notion of partial commitment to an independent central bank is not well developed. He provides two conditions under which the Bundesbank must give ground and follow a more expansionary monetary policy: when the pursuit of this policy does not threaten to ignite domestic inflation and when this policy enjoys overwhelming support within West Germany and abroad. The first condition is clearly ridiculous. All nontrivial examples of conflicts between the Bundesbank and the German government involve differences in opinion about the trade-off between benefits of economic stimulation and the costs of inflation. Nor does the second condition have much bite, as an example will show. In 1980/81, the Bundesbank followed an extremely restrictive monetary policy that contributed to an unprecedented level of firm bankruptcies and unemployment. The Socialliberal government, the trade unions, and all five national economic research institutes argued that the Bundesbank's policy was excessively restrictive. Nevertheless, the Bundesbank was able to withstand these pressures. (Indeed, its restrictive monetary policies are thought to have contributed to the collapse of the Socialliberal coalition in 1982.) In other situations, the Bundesbank was forced to accommodate government demands that did not have the overwhelming support of the German political and economic establishment.

Moreover, despite Goodman's ambition to analyze the role of institutional features that enhance central bank autonomy, his analysis is at times surprisingly insensitive to the monetary policy consequences of various institutional arrangements. For example, when summarizing the debate preceding the creation of the Bundesbank in 1957, Goodman is surprised to note that "no one directly called the independence of the future central bank into question. . . . At issue, instead, was the degree of centralization in the new central banking system." The written sources of the time, however, suggest that in the minds of at least some of the actors, the decentralized structure of the Bank Deutscher Länder (the Bundesbank's predecessor) was an institutional guarantee for its independence.

It is important to add that many of the limitations of Goodman's work regard questions that are unresolved in the literature and need to be addressed in future research, especially the link between institutions and commitment. Relative to the current state of the field, *Monetary Sovereignty* is a carefully executed and fascinating study of the politics of monetary policy and a must read for any scholar of central banking or European politics.

Stanford University SUSANNE LOHMANN

Peasants and Politics in the Modern Middle East. Edited by Farhad Kazemi and John Waterbury. Miami: Florida International University Press, 1991. 340p. $49.95 cloth, $22.95 paper.

Zimbabwe's Guerrilla War: Peasant Voices. By Norma J. Kriger. Cambridge: Cambridge University Press, 1992. 301p. $54.50.

Ethiopia: Power and Protest, Peasant Revolts in the Twentieth Century. By Gebru Tareke. Cambridge: Cambridge University Press, 1991. 272p. $49.50.

The study of peasant political activity is a well-established concern of comparative politics, and the peasantry as an analytic category has been the subject of considerable theorizing. Taken as a group, these three books (whose greatest similarity may be the word *peasant* in their titles) raise significant questions about this research focus. Reading them, one is led to ponder whether it is possible to speak of a distinctively "peasant mode of politics" and, indeed, whether the concept is of any clear theoretical utility. It would appear, as John Waterbury argues in the introductory essay of the work he has edited with Farhad Kazemi, that no single paradigm is adequate to explain the varieties of peasant politics. This need not imply, of course, that the study of peasants should be abandoned. As the clever title of Waterbury's contribution, "Peasants Defy Categorization (as Well as Landlords and the State)," also suggests, rural societies provide rich terrain for the study of political phenomena. If the peasantry as an analytic category is of questionable utility, there is nevertheless no doubt that studies of rural societies have much to contribute to the broader concerns of comparative politics.

Paralleling the field as a whole, many of the more interesting questions have centered on whether culturalist or rational-actor theories provide the more convincing explanations for political behavior. Within peasant studies this has been cast as the "moral versus political economy" debate, following James C. Scott's *Moral Economy of the Peasant* (1976) and Samuel Popkin's *Rational Peasant* (1979). In addition, there is a tension between "structuralist" approaches to peasant political mobilization and "voluntarist" ones, the latter of which concede a significant role to human agency in shaping political outcomes. Although devoted primarily to empirical case studies (which will be of significant interest in their own right to many scholars), all three of the works under review address, in different ways and to different degrees, these central theoretical concerns of comparative politics.

In many ways, Norma Kriger's *Zimbabwe's Guerrilla War* is the most intriguing of the three. While an initial chapter is dedicated largely to the voluntarist-versus-structuralist issue, her work may be most interesting for those concerned with the micro debate about what guides peasant political behavior. The book is based on fieldwork carried out during the brief period following Zimbabwe's independence, when Western social scientists enjoyed relatively easy research access to that country. The data she presents are a major part of the value of the work, and her extraordinarily candid discussion of the difficult conditions under which this data was gathered would be invaluable reading for any graduate student preparing for local-level fieldwork.

The initial question Kriger poses concerns the motivations for peasant actions: Why did Zimbabwe's rural populations participate in the guerrilla war? Her answer, in brief, is that they took part not for clearly utilitarian or material benefits but largely due to coercion, supplemented by cultural nationalist appeals. She thus purports to show, contra much of the literature, that revolutions can succeed without popular support. In presenting this argument, Kriger also provides significant material relevant to the separate question of *how* peasants participated, that is, how they responded to war-time conditions. Here, the answer is less straightforward. Peasant behavior, in her description, varied widely in accordance with localized circumstances and grievances. Critiquing the view of the peasantry as a homogeneous social category, Kriger differentiates within peasant society along various lines: gender, age/generation, structural or economic position, and local social cleavages, such as those separating migrants ("strangers") or royal lineages. These distinctions prove crucial to her interpretation of rural politics during war-"Peasant resentments against inequalities with other Africans provided even more powerful drives to participate in the nationalist guerrilla war than did peasant hostility to colonial policies and whites" (p. 51).

The story Kriger has to tell is vivid and complex. Indeed, the thread of her argument is occasionally obscured by the richness of her data. Among the things we learn in different places, for example, are that youth enthusiastically supported the guerrillas and resented them; that youth used ties to guerrillas to manipulate their parents, while parents relied on guerrillas to help control youth; that guerrillas used youth to extort from parents and kept good ties with parents; that guerrillas abused women and defended women from abuse by their husbands; that many guerrillas died of malnutrition and that they extorted food from peasants and thus ate better than under normal circumstances. In short, we learn that behavior was highly variable and idiosyncratic. This is of interest in its own right; yet aside from the general point that local and individual factors must be considered to understand each case, Kriger provides little to guide the reader in sorting out this confused set of events, motivations, and behaviors. Her account, nevertheless, raises intriguing questions for those concerned with explaining how (even rational) people will behave in high-risk circumstances with only very imperfect information and in the face of unpredictable behavior by others.

There are some notable flaws in the book. The several belabored passages detailing others' alleged theoretical and methodological failings contribute little, and the tendency to reductively dichotomize debates ultimately detracts from her own contribution. Such, for example, is the case when she lambasts the works of David Lan and Terence Ranger describing popular support for the guerrillas. There is no need to accept Kriger's dichotomy that peasants either supported the guerrillas or not. Surely, it is rather easy to believe that resentment against racist and colonial policies made most rural Zimbabweans sympathetic to guerrilla grievances, at the same time that the unpredictability and precariousness of peasant existence placed severe limits on their involvement, particularly when basic survival was threatened. Her work would be better presented as complementary and corrective to existing analyses, rather than as a refutation.

There are also some organizational difficulties and occasional contradictions. "Popular support" is at one time defined as "voluntary and shared by a significant *majority* of the population," while at another, only a *significant minority* is required (pp. 116, 160). Yet despite these shortcomings, the work is highly intriguing and provocative in its presentation of the case study. Even readers with little sympathy for Kriger's theoretical explanations will find themselves offering alternative explanations and formulating new hypotheses to interpret her data. This quality emphasizes the validity of her final point, the need for more primary fieldwork. The call is of particular relevance to the study of African politics, where many of the barriers to theorizing arise from the paucity of empirical knowledge. Kriger's book demonstrates that good fieldwork, independently of its analysis, still has much to contribute to the field of comparative politics.

Both Gebru Tareke's *Ethiopia: Power and Protest* and the majority of the contributors to the volume edited by Waterbury and Kazemi take a very different approach to studying the reasons for, and sources of, peasant political activism. In contrast to Kriger's micro look at motivations for peasant behavior, these works offer macro structuralist accounts of the sources of conflict in the societies they consider. Perhaps not coincidentally, they also largely conclude that each case can be fully understood only in light of its unique historical evolution.

Tareke situates his work, which he accurately labels a theoretically informed empirical study, within the "new Ethiopian historiography" that "gives primacy to social processes over individual actors" (p. 1). This structuralist orientation is laid out quite explicitly in his introduction: "Ideas are crucial, but material conditions and organizational possibilities may be even more decisive in determining whether peasants revolt or not" (p. 9). Although we are told at one point that "peasants are rational beings who take calculated risks" (p. 196), the work does not concern itself directly with the political-versus-moral-economy debate.

The core of the book is devoted to an examination of three cases of peasant insurrection in Ethiopia between 1941 and 1970: the Tigrean uprising of 1943, known as *Weyane*; the Bale revolt of 1963–70 involving the Somali and Oromo populations of southeastern Ethiopia; and the 1968 revolt in the predominantly Christian and Amhara northwest region of Gojjam. Thus, although they cover a relatively restricted time period, the three cases include very diverse ethnoregional settings. Tareke's structuralist perspective posits the particular salience of the historical moment and the social context of revolt. In part 1, he presents a superbly crafted account of Ethiopian state formation. Part 2 consists of sequential discussions of the three cases, aimed at answering the theoretical question he poses early on: "When and how does individualized rage and noncompliance coalesce into active collective defiance that may modify or alter power relations?" (p. 5).

The answer is not a simple one. While the outbreak of revolt ultimately depends on the specific local pattern of "the tangled web of social cleavages and shifting alliances," he also finds that "the three revolts were . . . similar in origin and outcome" (pp. 194–95). Tareke's concluding discussion of these similarities and differences invokes historical subtleties; and students of Ethiopia will certainly find much of value in the case study material, largely compiled from archival sources and interviews. But the work is also of both contemporary and theoretical interest. Tareke explores in an insightful (if somewhat curiously optimistic) epilogue how the current Ethiopian situation can only be fully understood in historical context. Moreover, the work's attention to the development of the modern Ethiopian state as a central structural factor shaping conflict will be of particular interest to scholars working within the state-centric paradigm in comparative politics. Tareke's study will no doubt serve as a valuable source for subsequent comparative studies of revolution and conflict.

The best of the essays in *Peasants and Politics in the Modern Middle East* will likewise be of broader interest; but taken as a whole, the work suffers from many of the chronic difficulties of edited collections. In addition to Waterbury's introduction, the book contains 13 essays, most of which are case studies of rural mobilization in a wide array of Middle Eastern states. The authors represent Middle East specialists from a number of academic disciplines, and the approaches they take to the individual cases thus vary widely. Because the majority, constrained by the available historical data, tend to take a macro approach to the analysis of the individual cases, the explanations offered are most often of the structuralist type. At times, the theoretical debate about the micro factors shaping peasant behavior is also invoked.

Thus, Donald Quataert, in his chapter on the Ottoman territories, tells us that the relative peacefulness of nineteenth-century Anatolia compared to Bulgaria or Bosnia–Herzegovina "stemmed partly from the moral economy of the peasantry in Anatolia, a culture disinclined to use violent protest to register its discontents" (p. 43). By contrast, Farhad Kazemi's discussion of three movements in Iran, Iraq, and Turkey is based on the assumption that "to partake in collective violence is ultimately a choice that peasants make based on some rational calculation of costs and rewards" (p. 101). The problem with such assertions is simply that they are just that. In the brief space of the essays, little or no evidence is presented to allow the reader to evaluate the validity of these theoretical claims.

The book also contains three interesting contributions of a somewhat different nature. Nathan Brown's tantalizing discussion of peasant "ignorance" lends itself either to the view that the behavior provoking that critical label is a cultural feature of peasant society or that it is a rational response to state power; Timothy Mitchell, in a deconstructivist mode, analyzes various texts to study "the representation of rural violence"; and Nicholas Hopkins, an anthropologist, provides an interesting comparison of the emergence of local-level class structures in two Arab villages.

One general theme of the volume, sketched out briefly in Waterbury's introduction and in the contribution by Edmund Burke and reflected in the roughly chronological arrangement of the case studies, is the idea that there has been a broad historical evolution in the nature of relations between states and rural societies and that at least in the Middle East, the nature of peasant politics has, consequently, varied over time. This insight leads Waterbury to suggest that "as we near the end of the century, it is probably no longer useful to speak of peasants in [Eric] Wolf's sense of the term" (p. 18). Waterbury's emphasis on the difficulties of categorizing peasant politics no doubt arises, at least in part, from the eclecticism of the contributions to his volume. But considering all three of these books together, one is led to question the theoretical utility of a category as broad as "peasants."

University of Kansas LEONARDO A. VILLALÓN

The Politics of Nationalism and Ethnicity. By James G. Kellas. New York: St. Martin's, 1991. 188p. $45.00.

In this ambitious new work, James G. Kellas sets himself the daunting task of providing the reader with an "integrated theory of the politics of nationalism and ethnicity" (p. 6). By so doing, the author also hopes to introduce and explain "all the major approaches to the subject" (p. 2). In Kellas's view, such an approach is absolutely vital because "without such a theory, it is impossible to make a claim that understanding has been advanced beyond the level of the description and partial analysis of apparently unrelated case studies" (p. 159). While the volume falls short of achieving its grander theoretical objectives, it does succeed in fulfilling the more modest goal of providing a clear and relatively comprehensive introduction to this vast body of literature. As such, it would be of greatest use to the novice who is coming to the topic for the first time or to the more experienced scholar who is seeking a work that in synthesizing the contributions of others both outlines the issues of greatest concern to the field and underlines the relations between them.

The strength of the book lies in its straightforward exposition of the classic questions that have been asked (if not definitively answered) by those who have studied nationalism: What is its relation to human nature and the psychology of social groups? What are its intellectual origins, and how has it evolved conceptually? What are the conditions that historically favored its emergence, and were there (to quote Armstrong) nations before nationalism? What political and social forms has nationalism taken, and how does it interact with politics, economics, and culture? Which classes give rise to nationalists and which provide them with their bases of social support? Which political forms are most conducive to accommodating ethnic demands in divided societies? In addition, Kellas pays attention to issues that have received less systematic attention by political scientists—for example, the influence of nationalism on international relations and foreign policy and the pervasive presence of nationalist behavior outside the political realm. There is only one important lacuna in this otherwise comprehensive introductory tour: Kellas gives sparing attention to the important relationship between the military and ethnicity.

The concern for definitional clarity and the judicious use of typology only further enhance the accessibility of the subject matter and Kellas's argument. On occasion, however, this definitional rigor deserts the author. For instance, in his discussion of régime change, Kellas departs from standard conceptions involving transitions in regime type or, at minimum, changes in political leadership, stretching the concept to include, in one case, the modernization that resulted from Quebec's "Quiet Revolution" of the 1960s. Yet while the author's approach provides the ideal foundation for a review of the literature on ethnicity and nationalism, it cannot bear the weight of his wider theoretical claims.

To achieve the integrated theory that is Kellas's chief ambition would have required the author to alter his analytical stance—moving from his role as nonpartisan observer to that of theoretical advocate. This he appears unwilling to do, since he derides other theorists, with some justification, for denying "the truth of theories other than their own" (p. 160). While Kellas freely admits his debt to the work of others, he defends the originality of his contribution combining "these into a general theory in which the 'building-blocks' are clearly displayed, and in which 'necessary' and 'sufficient' conditions for aspects of the politics of nationalism and ethnicity are identified" (p. 160). What this eclectic approach ultimately produces, however, is less an "integrated theory" than an integration of theories. Missing is a statement of general principles and assumptions (coherent, connected, and consistent, with implications systematically developed and investigated at successively lower levels on a ladder of abstraction. It is not so much that Kellas's theoretical treatment fails to descend such a conceptual ladder as that in doing so, it lacks a unifying focus other than its topic. In metaphorical terms, the author's building-blocks are more cemented in place than chiseled from the same stone.

This is not necessarily a fatal liability. Kellas's work is still important, introducing the reader to the subject in all its complexity, presenting the theories put forward to account for its various manifestations, and judging when and in what conditions those theories' insights are most appropriate. This is no mean feat and is probably even more rewarding than pursuing the chimera of general theory. That said, the book would have been greatly improved if the author had presented his theoretical synthesis before tackling the case study on nationalism in the First, Second, and Third worlds. Such a presentation would have enabled Kellas both to reinforce his theoretical insights and enrich his empirical narrative.

Columbia University CHARLES F. FURTADO, JR.

Burying Lenin: The Revolution in Soviet Ideology and Foreign Policy. By Steven Kull. Boulder: Westview, 1992. 224p. $34.95 cloth, $15.95 paper.

Moscow and the Global Left in the Gorbachev Era. Edited by Joan Barth Urban. Ithaca: Cornell University Press, 1992. 224p. $29.95 cloth, $12.95 paper.

The Gorbachev revolution in Soviet foreign policy was inextricably linked to ideological change within the Communist Party of the Soviet Union (CPSU). The Leninist construct of international class conflict and anti-imperialism was replaced by a desire for Soviet integration into the world economy and by the realization that international cooperation was necessary to overcome the global threats of nuclear war and environmental catastrophe. Two new books explore this ideological revolution and its consequences.

Steven Kull's well-written book (part of a major project at Global Outlook, a research organization based in Palo Alto) dubs the Gorbachev era a "Kuhnian revolution," a scientific breakthrough that produced a new analytic paradigm for Soviet foreign-policy makers. He argues that objective factors, such as Soviet economic stagnation, are alone insufficient to explain the radical turnaround in Soviet foreign policy. Instead, he writes, analysts must examine such factors in light of the cultural meaning assigned to them.

Kull's descriptions of ideological change and its foreign policy effects are right on the mark. Soviet area specialists may not find Kull's data surprising; but the quotations he includes from Global Outlook's interviews with Moscow elites provide riveting insights into the thoughts of people whose worldviews were crumbling around them. Kull uses his training as a psychologist to reveal the internal struggles CPSU leaders faced. Many Soviet communists came to believe that a mixed market system led not only to the superior economic performance of Western countries but also to a better life for the very working class that they, as Marxists, lauded. Kull also provides an in-depth look at the dilemma faced by those who let go of their communist ideals but retained their belief that the Soviet Union should remain a superpower. That theme resonates today, as Russian President Boris Yeltsin faces conservative opposition to his conciliatory foreign policy initiatives.

Joan Barth Urban's edited volume originated as a panel presentation at the Fourth World Congress of Soviet and East European Studies in Harrogate, England. Chapters are written by Heinz Timmerman, Harry Gelman, S. Neil MacFarlane, and Federigo Argentieri, in addition to two substantive articles by Urban herself. This book, too, examines the convergence of CPSU thinking toward Western norms. However, its focus is unique: it portrays the Soviet move away from Leninism as a move toward the social democratic half of the international workers' movement of the early twentieth century. Urban and her colleagues argue that Soviet leaders sought to reunite the split socialist movement by giving up hardline dogma. This book is especially valuable because several of its authors provide descriptions of how individuals at particular institutions (e.g., the CPSU Central Committee Institute of Social Sciences and the editorial staff of the journal *Problems of Peace and Socialism*) worked to promulgate the new ideas among the leadership. We can see not only what "new thinking" was but how it may have spread.

Timmerman opens the volume with a detailed overview of Soviet attempts in the late 1980s to join democratic and socialist values. In particular, he notes, CPSU leaders praised the welfare state and the environmental accomplishments of the West European Left, as well as democratic socialism's esteem for individual rights. Each of the volume's other authors then explores in depth how the rejection of hardline Leninism affected Soviet relations with leftist actors in particular geographic areas: Italy, China, the Third World, and Eastern Europe. Urban's concluding chapter makes the noteworthy claim that the abortive August 1991 coup attempt reflected the schism between Leninist and democratic socialist camps within the CPSU.

Both of these books are thoroughly researched and engaging. Both provide new insights into the ideas of the Gorbachev revolution. Both raise one question, however, to which no complete answer is given: Did ideological change cause policy change, or did something more fundamental cause both ideology and policy to be altered? (Some specialists have suggested that generational change was responsible, while others have focused on socioeconomic dissatisfaction caused by the Soviet defense burden.)

While there is abundant evidence that ideological change happened, it is difficult to demonstrate its causal role. Kull argues that policy changed when Soviet leaders began to perceive the impact that mutual nuclear vulnerability and economic interdependence had on global relations. What remains unclear is why this perception took hold when it did, and not earlier. Timmerman argues that it was the democratic impulse of perestroika, as well as a desire to join the world community, that led to an ideological reexamination of socialism. Yet we are left wondering what caused these impulses to arise. Gelman's chapter in the Urban book describes the deideologization of Soviet–Chinese relations as an instrument of rational policy goals, rather than as a cause of policy reformulation. Perhaps, then, ideology is a by-product of policy, rather than a cause of policy choices. MacFarlane argues that a variety of causal factors all shifted at once and that it is impossible to disentangle the effects of international pressure, domestic hardship, historical reevaluation, and personnel change on policy.

In the bad old days, Sovietologists were divided by their views on whether communist ideology or realist power considerations motivated Soviet foreign policy behavior. Under Gorbachev, a new globalist ideology replaced the old one; and analysts continue to debate its roots and significance. Today, this debate may be even more meaningful, as analysts try to predict anew what direction Russian foreign policy will take in the long run.

This debate serves to link Soviet and post-Soviet studies to mainstream work in international relations, where many analysts have now turned toward examining the relative impact of objective versus interpreted structure on state behavior. Both of these books help us to understand how ideology has conditioned the thinking of Moscow elites and help to carry the debate forward. Both will be especially useful as readings in courses on Soviet and post-Soviet foreign policy.

Ohio State University KIMBERLY MARTEN ZISK

Islam and Human Rights: Tradition and Politics. By Ann Elizabeth Mayer. Boulder: Westview, 1991. 258p. $39.95.

Human rights are a hotly debated issue in the Islamic world, although the only side of this debate to enter American consciousness, largely uncritically, is that presented by the more authoritarian rulers. ("You don't understand our ways; in our country it's all right if we beat up people.") Ann Elizabeth Mayer's book engages this debate by examining the arguments about human rights presented by the most zealous defenders of the region's few Islamic regimes: Iran since the 1979 revolution, Pakistan under Zia (1977–88), and the Sudan in Nimeiri's last years (1983–85).

Mayer's central point is that the protections that various Islamic human rights schemes offer, relying on Islamic principles and sources, systematically limit, rather than protect or expand, the sorts of civil and political rights found in the Universal Declaration of Human Rights and related international law. Although some Islamic writers (sometimes disingenuously, she suggests) disguise this fact, Islamic human rights schemes restrict, rather than expand, freedom. They do so not primarily because of something inherent in Islamic teachings but usually in the service of dictatorial rulers who selectively interpret Islamic thought in a way most consistent with continued dictatorial rule. Mayer does not find the arguments that Islam necessarily restricts rights convincing. Nor, evidently, does the political public in these Islamic states. If they did, they would not resist; and regimes would not find human rights violations necessary. Genuine tradition enforces itself through moral, not physical, sanction. Muslims do not generally buy these arguments; and neither, she suggests, should we.

Mayer examines specific efforts to codify Islamic positions on human rights issues found in the Iranian Constitution, the short *Human Rights in Islam* by Abu'l A'la Mawdudi (founder of Jama'at-i-Islami), the Azhar-affiliated Islamic Research Academy of Cairo's *Draft of the Islamic Constitution*, the Universal Islamic Declaration of Human Rights, and (because of its unusual transparency) Sultanhussein Tabandeh's *Muslim Commentary on the Universal Declaration of Human Rights*. She devotes special attention and separate chapters to issues related to the treatment of women and non-Muslims, whose rights these schemes most systematically restrict. One could quibble about her cases: only one state is Arab, and none of the documents are specifically Sudanese; but other documents she might well have chosen would reveal essentially the same pattern as these do.

Mayer attributes this restrictive pattern to two factors. First, and overwhelmingly, she attributes it to the cynical appeal to religious sentiment by rulers, aided by conservative clerics, in an effort to legitimize fundamentally unpopular rule. Of the many interpretations of rights consistent with Islamic teachings from which they could choose, these unelected rulers favor those granting the state the most authority. This conclusion—that rulers manipulate Islam—is certainly true and needs to be heard by American audiences. Second, and more implicitly (indeed, despite some arguments to the contrary), she does seem to give some independent weight to Islamic teachings themselves. She states clearly that these teachings do not dictate one position on human rights, that they are very flexible. (Islamic thought today, for example, rejects slavery, although the Quran condones it.) Yet the underlying theme of the internal chapters is that the small but hard core of Islamic thought may well be incompatible with equal protections of some important rights, at least for women and religious minorities.

This ambivalence about the independent role of Islamic teachings may be rooted in the one recurring problem in the book (more a distraction than a fundamental flaw), namely, the uncritical acceptance of modernization theory and its categories (not Shari'a but "premodern" shari'a, the "development process," "transitional stages" en route to modernization, etc.). The literature these terms evoke has largely, and appropriately, been discarded by scholars outside Mayer's discipline (international law). Not only is the literature's underlying assumption (that one can identify internally homogeneous but mutually exclusive worlds of tradition and modernity connected by one development process) wrong, but, more importantly, its implications that teachings, simply because they are from a "traditional society," are, if not static, at least inappropriate in today's world. If more Islamists choose not to reconcile their beliefs with the Universal Declaration of Human Rights, the reason may lie less in the fact that Islamic teachings were originally formulated in a different kind of society (one, she argues, that favored the collective over the individual) than in political calculations—perhaps a need to distinguish themselves from the secularists, progressives, and (in the Arab world) the old Arab nationalists, who have staked out the human rights issue. They need to distinguish themselves precisely because human rights activists and Islamists share so many goals (to constrain arbitrary rulers, to replace arbitrary rules with predictable laws, to introduce accountability) yet must compete for the popular support of the same groups. Islamists, after all, have a real interest in supporting human rights protections. In the Muslim world generally (her cases are the exception), Islamists are not in power. They are, rather, the target of state violence, more often the victims than the perpetrators. Indeed, many do support broader interpretations of human rights. If they are reluctant to embrace the issue more completely, the reason probably lies in the political realm.

This is a brave book. Some people will read her criticism of regimes as a criticism of the people who live under them; others will fault her for criticizing this regime but not that one. The first criticism would be inaccurate, the second unfair. Mayer's contribution lies in bringing the central issues of a debate about rights important in the region but little known outside it to a larger readership and in criticizing the one side of the debate that we normally hear. In doing this, she makes an important contribution to issues of rights and cultural relativism relevant to discussions well beyond the region.

University of Michigan JILL CRYSTAL

The Political Economy of Basic Human Needs. By Bruce E. Moon. Ithaca: Cornell University Press, 1992. 316p. $45.00 cloth $14.95 paper.

Basic needs—abstractly defined as those minimally required to sustain life at a decent material level—are represented in this cross-national study by a scale of infant mortality, life expectancy at age one, and the literacy rate. Basic needs reflect the social well-being of the population without attention to such messy, sometimes unmeasurable concepts as poverty, equality, nutrition, and fairness. Unlike income and wealth, infant mortality, life expectancy, and literacy reflect absolute, more than relative, inequality; cannot be monopolized by the elite; and refer directly to outcomes, rather than inputs.

The focus on political economy in the study translates into analysis of the influence of broad-scale national economic, social, and political characteristics. Moon criticizes naive economic determinism that takes fulfillment of basic needs as a direct and automatic consequence of economic development. That basic needs vary independently of economic development raises the theoretically interesting question of why some nations do better than would be expected from their financial circumstances. Moon considers, one by one, the independent influence on basic needs performance of the state and political system, position in the world system, and colonial history. Although the causal interrelationships among variables mediating development and basic needs makes separating direct and indirect causal effects of all variables difficult, support for the political economy perspective comes from demonstrating influence of political and world system variables net of development.

The analysis of the national economic, social, and political determinants of the scale of infant mortality, life expectancy, and literacy—termed the Physical Quality of Life Index (PQLI)—relies on aggregate data for 120 nations. Unable to examine the consequences of specific policies, the study examines aggregate statistical relationships. Moon shows a care and thoroughness that are often lacking in such analyses. Problems of data reliability, model robustness, and time lags inherent in cross-national statistical analyses receive much attention. The high ratio of text to tables reflects commitment to underlying theory and argument, careful justification of the measures and models, and avoidance of crude empiricism.

Two themes emerge from the analyses. The first is that nations may take different paths to economic development, some of which fulfill basic needs more rapidly than others. Liberal economic theories that treat development as unidimensional come under criticism. For instance, uneven development isolated in a small number of export-oriented sectors ("disarticulated development" in the jargon of dependency theory) slows fulfillment of basic needs. Accordingly, the models show that the sectoral distribution of the labor force better predicts the PQLI than national income. Capital-intensive industrialization combined with mining or subsistance agriculture does less for the basic needs of the population than labor-intensive industrialization, commercial agriculture, and a plantation-based farm organization.

The second theme concerns the ability of the state to overcome developmental barriers to basic needs improvement. Briefly, Moon finds that components of the state influence the PQLI independently of economic controls but not in simple, automatic ways. Regime ideology and government expenditures do not alone increase or reduce basic needs, but a rightist government in combination with high expenditures lowers basic needs. Military spending reduces basic needs by directing resources away from social uses, but military manpower improves basic needs by exerting a modernizing influence on the lives of former soldiers. Democracy improves basic needs, but so does totalitarian state socialism. Easy generalizations that neatly fit standard ideologies do not emerge from these analyses of states and politics.

Despite the care Moon gives to the robustness of the statistical results and the validity and reliability of the measures, he could have done more. Nearly all analyses mix high-income and Third World nations together; one could have more confidence in the observed relationships were they shown to hold within the subsample of Third World nations. Given the difficulty in separating the influence of the constellation of economic, social, and political characteristics that distinguish high-income nations from others, multicollinearity diagnostics would have been appropriate. Further, the importance attributed to state socialism on the basis of 11 nations with suspect data causes some concern.

The Political Economy of Basic Needs makes a clear contribution to the cross-national literature on the inequality, poverty, and social well-being of national populations by presenting a clear and thorough analysis of the data. The detailed discussion of the causal mechanisms behind predicted and observed relationships and the careful (if in some ways incomplete) statistical analyses of the relationships much improve on previous work in the field. The theoretical contribution is less clear, in part because of a lack of generalizations that fit into standard theoretical categories. The critique of liberal economic theories of comparative advantage should resonate with political scientists and sociologists, but this audience needs little convincing on the point anyway. Despite partial reliance on theories of world systems and dependency, the alternative perspective is defined more residually in response to classic modernization theories. However, the findings from this book should be useful for future research toward constructing a systematic theory of public policy that makes sense of the diverse, multifaceted influence of state and political variables.

University of Colorado FRED C. PAMPEL

The Presidency in Mexican Politics. By George Philip. New York: St. Martin's, 1992. 230p. $59.95.

In the shadow of the Salinas administration's effort to write still another chapter in Mexico's saga of political authoritarianism, Philip's study explores the character of presidential rule during four previous administrations, those of presidents Díaz Ordaz, Echeverría, López Portillo and De la Madrid. Philip's essay considers the nature of Mexico's presidential institution, focusing, in particular, upon the psychological dispositions, leadership style, human relations skills, and political acumen of Mexico's leaders, along with the substantive content of their political choices. The study attempts to map presidential relations with organized social interests and tries to determine the sources of constraint upon presidents and the margin of their room for maneuver, suggesting a profile of the distribution of power in the system.

Philip's study offers a richly detailed portrayal of the personal characteristics and orientations of recent presidents that helps to convey a feel for the texture and tone of Mexican presidentialism, doing so most successfully perhaps in his fascinating discussion of President Díaz Ordaz. The author argues that the Mexican system has changed a great deal over the past quarter-century and maintains that there has been at least a "partial unravelling" (p. 17) of the Hobbesian bargain that was struck during the Cárdenas period to control political mobilization. While in the past, the system has, in Philip's view, offered "marginality for the poor, corporatism to the middle sectors and neopluralism to the intellectuals and the bourgeoisie" (p. 165), the system's real problem is its inability to deal with alternative elites. Yet Philip holds out the prospect that Mexico might successfully continue its evolution in the direction of a competitive party system, retaining some form of societal corporatism (as it now appears to be doing) and thereby avoiding a wrenching political upheaval.

These rather sanguine conclusions notwithstanding, Philip's study actually details the continual efforts of recent Mexican presidents to reassert "despotic power" and finds these efforts to have been largely counterproductive, leading to the setback of what he terms "infrastructural objectives" (p. 173). At the same time, Philip contends that Mexican presidents must find a way to exercise skillful political leadership and avoid what he terms a "nonpolitical" style of presidential leadership, in view of the flawed character of other Mexican political institutions. Unfortunately, Philip's portrayal of presidential behavior suggests that the principal motive behind the exercise of presidential power over the last several administrations has been the preservation of presidential power itself. Mexico's presidents are motivated, it is argued, precisely by perceived threats to their power, whether emanating from the Left or the Right.

While the study offers considerable insight into the personal motivations that may have stirred Mexico's leaders, the author does not attempt to link these observations to comparative work or to frame them with reference to studies of elites, political leadership, or more general studies of political psychology or decision making. Instead, discrete observations about presidential style ("extreme presidentialist" for Díaz Ordaz, megalomaniacal for Echevería, consensus-making and later borderline delusional for López Portillo) are offered. One worries most especially about the absence of a conceptual framework for interpretations of leadership style and personality and of any other theoretical framework when the author attributes the entire underlying pattern of the De la Madrid administration to "presidential personality itself," rather than, for example, to international factors, societal conflicts, or growing domestic political conflicts (p. 135). Despite these reservations, Philip opens up stimulating lines of interpretation of recent presidential orientations that have been too often overlooked in analysis of Mexico's economic and political development. In particular, Philip's study retrieves important lines of recent societal development from the sometimes barren reaches of policy analysis and reestablishes their genesis within a political context not always framed by reasoned calculation beyond that of personal advantage and personal discretion.

University of New Hampshire JUDITH GENTLEMAN

The Reemergence of Civil Society in Eastern Europe and the Soviet Union. Edited by Zbigniew Rau. Boulder: Westview, 1991. 181p. $39.95.

Civil society in its liberal sense, Rau tells us, "is a voluntary association of individuals and their associations participating in political life and forming a moral community" (p. 138). As Kiss (1992) noted in *Dissent*, *civil society* not only has been a catchword for dissidents during the last 20 years but "provided a powerful theoretical tool for criticizing communism, as well as a successful strategy for undermining it." In East European usage, the term has mixed the abstract, the moral and the practical, much as such different levels are mixed in the West when we discuss "democracy."

The four case studies in this book examine the role of civil society in fostering nationalism among Ukranians, Estonians, and Crimean Tatars and in illuminating the role of the Polish press in 1981, providing practical examples of what Konrad called Anti-Politics. These minihistories of submerged channels of information and mobilization, written by involved intellectuals and journalists of the respective movements, see no incompatibility between the rise of nationalism and the (re)emergence of civil society.

The moral and civil aspects of civil society are examined in Rau's chapter on human nature and social engineering, where he contrasts Leninist attempts to make the New Man with perceptions of such efforts provided by Alexander Zinoviev, Czeslaw Milosz, and the Czech dissidents. *Homo sovieticus*, Rau suggests, failed because "the social engineer can only *undermine* human beings' rational and moral capacity, not *deprive* them of it" (p. 46). One hears Vaclav Havel here, as well as echoes of Montesquieu and Rousseau. Yet in the recent civil society debate, Ed Shils (1991) has noted, "The association between virtue and republican governments has been passed over" Abstract discussions of civil society are found in Rau's introduction; Heltai and Rau's reflections on nationalism, civil society, and tolerance; and chapters by John Gray and Andrew Arato.

Although contributors to the confusion of meaning associated with *civil society* include virtually every major political and economic theorist since Adam Smith, the major culprit is certainly Hegel. Yet the autocratic kings and *bürgerliche Gesellschaft* of Hegel's day are not the Communist one-party rule of the twentieth century, nor did the two seek the same kind of legitimation. Rau goes somewhat astray under this influence, asserting that "in the interaction between civil society and the state, both constitute distinct entities that have distinct domains outlined by firm boundaries" and claiming that the Soviet-type system "resembled the Hegelian constellation" (p. 6, 9). Hegelian mystifications of the separate, Universal-embodying state and the Marxist–Leninist conglomeration of centralized one-party economic control acting in the interest of the working class have a rather tenous relationship to each other, especially since Marx went out of his way to reject Hegel's conception of the state. Hegel's society was interdependent with, but subservient to, the guardian state; Marx's society would completely subordinate the state.

The trouble for the post-Soviet world is that the Party–state completely subordinated civil society. Civil society can only exist in a productive tension with the state, suspended between private, familial concerns and public, governmental ones. The reemergence of civil society runs the danger of delegitimizing the very political institutions it needs for survival.

Gray's conservative solution urges ex-communists to look to nondemocratic Asian examples (e.g., Taiwan) to see how one can enforce the rule of law and protect a market economy. Arato, on the contrary, suggests that one look at the defects of the Western liberal democratic model before undertaking any rash reforms. Rau himself explores a Lockean contractarian approach. And Heltai and Rau suggest that one means to move to a liberally oriented civil society and away from "the parallel wave of nationalism" (p. 129) is to institutionalize local national communities along the lines of Swiss cantons.

Civil society has attracted renewed attention. Witness the contributions of Adam Seligman and John Keane and the shorter reflections of Ernest Gellner (1991) and Michael Walzer (1991). This volume is essential to the continued discourse, particularly in its ability to focus attention on the efforts to create new participation in political life in Eastern Europe and the Soviet Union—the more valuable at a time when Western participation is so questioned.

Bryn Mawr College JOHN BENDIX

State and Society in China: The Consequences of Reform. Edited by Arthur Lewis Rosenbaum. Boulder: Westview, 1992. 240p. $43.00 cloth, $15.95 paper.

Popular Protest and Political Culture in Modern China: Learning from 1989. Edited by Jeffery N. Wasserstrom and Elizabeth J. Perry. Boulder: Westview, 1992. 300p. $49.50 cloth, $19.50 paper.

These volumes lay bare many of the frustrations that have been plaguing China scholars since the tanks rolled through Beijing in June 1989. Caught by surprise by major developments from the Great Leap Forward to the arrest of the Gang of Four, China scholars were also unprepared for the nightmare of June 4.

Most of the essays in *State and Society in China* are part of long-term research projects that were under way well before the Tiananmen crisis. The contributors were impressed by various aspects of the economic, social, and political progress of the post-Mao era. They were acutely aware of the serious problems that confronted the reform effort but remained hopeful, nonetheless. Most of their research began as attempts to analyze the reform decade. But bullets began to fly before any definite conclusions could be reached. As a consequence, by the time the volume went to press, the discussion of reform became less problematic than explaining why the reforms were followed by such an ugly episode of violence and repression.

Explaining the Chinese case was complicated by the unanticipated collapse of state-socialist systems throughout Eastern Europe. Why did the socialist system vanish in Eastern Europe but not in China? Why did the Chinese reform movement spin out of control after making such a promising start? Did it have something to do with the "civil society" variable? Why was China different? This book offers no clear answers to these nagging questions.

State and Society in China reveals that under present circumstances, China scholars are understandably risk-averse. No one wants to make rash predictions when the situation is so volatile. The anticlimatic interpretation that prevails in these essays is that the reform era was an awkward mix of success and failure. The scholars agree, however, that even with the harsh repression that followed June 4, there can be no return. The reform initiative may not deepen very much, but neither can it easily be rolled back.

As to the fate of the regime, this group prefers to hedge its bets. Since no one argues that the reforms (many of which limited the reach of the state) were a total failure, no one rules out another crisis (especially in the structural realm) and the eventual collapse of the system. But given the impressive resilience of the party–state and the admittedly piecemeal nature of efforts to reform traditional Leninist political arrangements, no one is ignoring the staying power of the regime. In a perceptive essay on rural China, Thomas Bernstein says, "Chances are that the country will continue to muddle along, unable to do without the reforms but also unable to fully endorse them" (p. 161). In some respects, this is the cautious tone adopted by all the contributors.

These scholars are social scientists with generally a structuralist approach to the study of China. Some, like Lowell Dittmer and Hong Yung Lee, privilege the party–state by placing enormous attention on the role of political elites. Others examine the role of nonelites but focus, nevertheless, on structural dynamics. They rely upon data provided in official sources and are interested in how new policies formulated at the top alter political and social life at the bottom. In this sense, the strong essays by Martin Whyte on cities, Andrew Walder on workers, Dorothy Solinger on entrepreneurs, Stanley Rosen on students, and Merle Goldman on intellectuals fit within a well-established tradition of scholarship. Very little is said about culture and cultural issues, and few questions are raised about how popular protest movements challenge the hegemony of the party–state.

Popular Protest and Political Culture may not qualify as social science in the eyes of some readers, but it is the more adventuresome of the two volumes in interpretive and methodological terms. Most of the contributors are neoculturalist historians who have a strong interest in the problems of contemporary Chinese society. According to the editors, the Tiananmen crisis defied the predictions of mainstream social scientists and thus forced the academic community to reconsider many of its basic assumptions about the relationship between state and society. The contributors by no means express one view on how the events of 1989 should be interpreted, but they do agree that it is crucial for scholars to pay more attention to the role culture plays in shaping political action.

Some readers will be surprised to discover that this volume breathes new life into the discredited notion of "political culture." Pioneered in the 1950s by Karl Wittfogel and developed in the 1960s and 1970s by Lucien Pye and Richard Solomon, theories about Chinese political culture were subjected to considerable ridicule. But as coeditor Elizabeth Perry points out, the neoculturalists do not use the concept of political culture in a static, deterministic way. Culture is defined here as a dynamic force that is rooted in history and constantly changing. In brief, they argue, the crisis of socialism in China has been shaped in part by distinctively Chinese modes of political–cultural interplay. Those who rely excessively on the comparative communism approach to understanding China, these essays suggest, will fail to appreciate how the evolving political culture of China shaped the outcome of recent protests. In cultural (as opposed to structural) terms, socialist China was different from socialist Czechoslovakia. Thus, the course of popular protest was different.

The emphasis here is on explaining the failure of the recent protests. Such contributors as Ernest Young, John Israel, Vera Schwarcz, Timothy Cheeck, and Stephen MacKinnon do this by pointing to the many ways in which the failures of the 1980s were foreshadowed by trends in political culture that surfaced in the late Imperial, Republican, and early Communist eras. The most innovative and suggestive essays in this volume are the ones that make use of conceptualizations normally associated with literary studies. Most notable are the marvelous chapter by Joseph Esherick and Jeffrey Wasserstrom on "political theater," the superb essay by Elizabeth Perry on the "casting" of the democracy movement, and a provocative concluding study by Jeffery Wasserstrom that discusses the dynamics of myth making and myth breaking in the political arena.

Neither the structuralists nor the neoculturalists are very optimistic about the likelihood of fundamental political change in the near future. The neoculturalists, however, seem to be more pessimistic. This may be because they are looking at popular movements for signs of hope. By contrasting the success of popular protests in Eastern Europe to the apparent failure in China, the neoculturalists seem to be saying that there is something about Chinese political culture (however defined) that ultimately blocks substantive political change.

Perhaps there is too much attention given in *Popular Protest and Political Culture* to the connection between Chinese political culture and the failures of 1989. The culturalistic explanations for the failures of the protests are intriguing, but one wonders whether it is too early to speak of "failure." Despite disclaimers to the contrary, one gets the feeling from reading these essays that the failure of the popular protests was inevitable. As always, students cannot unite with workers, intellectuals have contempt for peasants, and men have little respect for women. The comparative communism approach has its demerits; but if nothing else, it teaches us that there were many failures in Eastern Europe and the Soviet Union before the collapse of socialism. Is there nothing in Chinese political culture that points to something other than failure? This volume shows that popular protest movements do not yet offer a viable alternative to the party–state; but it loses sight of a more important point, namely, that counterhegemonic trends in popular culture do not have to be successful in a programmatic sense to be able to subvert the party–state.

The structuralists may ignore the cultural variable; but they repeat often in *State and Society in China* that none of the social, political, economic, and other "structural" problems that were festering in China before the June 1989 crisis have disappeared in the post-Tiananmen era of tight control and repression. Perhaps these scholars do not give us much insight into how popular movements from below might topple the regime, but they remind us that authoritarian systems sometimes collapse under their own weight long before there is a well-organized, popular, democratic opposition.

University of California, San Diego PAUL G. PICKOWICZ

Rethinking the Center: Party Politics in Nineteenth and Twentieth Century Chile. By Timothy R. Scully. Stanford: Stanford University Press, 1992. 287p. $42.50.

This book addresses some recurring questions about Chilean party politics, specifically its origins and evolution, its stresses and crises, and the roles played by "center parties" in the overall Chilean political processes. Two basic analytic concepts are employed by the author: *cleavages* and *critical junctures*. The analysis draws for general themes on mostly familiar works by David Collier, Ruth Berins Collier, Seymour Lipset, Stein Rokkan, and Giovanni Sartori and for Chile in particular, on works by Arturo Valenzuela and Samuel J. Valenzuela. Research in Chile was conducted by the author primarily from 1986 to 1989, although he also served as a Catholic priest in Santiago between 1979 and 1983. The analysis covers the period from approximately 1857 to the military coup of 1973, with a brief updating of current redemocratization politics in Chile. While a good deal of party and electoral data is included, the analysis is more an attempt to conceptualize a complex and often confusing experience and relate it more generally to ideas about the evolution of party politics. The scope and tasks the author defines of conceptualizing, interpreting, and illustrating 12 decades of Chilean party competition are formidable, especially when limited to a discussion of only 202 pages; but to a large extent, the effort is successful. The interpretation is carefully argued, refreshingly free of ideological paraphernalia, and generally persuasive.

The author identifies in Chilean politics three fundamental cleavages and three critical junctures, the latter being the point at which an emerging cleavage redefines party competition. The first cleavage identified was a clerical/anticlerical one whose critical juncture was 1857–61, producing a tripartite party system with the Liberals as the center, which persisted till 1920. The second was an urban-sector class cleavage whose critical juncture was 1920–32, realigning the party system with Marxists on the left, the radicals in the center, and traditional conservative parties on the right; this system persisted till 1952. The final, decisive cleavage was the rural-sector class, whose critical juncture was 1952–58, ultimately producing a party system in which Christian Democrats dominated the center from 1963 onward. The subsequent analysis describes, argues, and attempts to justify these defining cleavages and their critical junctures, explaining their political consequences, particularly the influence of the center parties, a subject, the author argues, that needs rethinking. He concludes that center parties in the ideologically polarized context of Chile have been of two types: those which have mediated and arbitrated between the two poles for the purpose of achieving and sustaining power and those which have their own programmatic objectives.

One could quibble over some of the specific interpretations and conclusions in the text; but the strength of the book is that it provides a succinct and plausible conceptualization for the evolution of Chilean party politics and identifies the crucial role of center parties during three critical junctures in the process. He persuasively argues that the failure of the Christian Democrats to arbitrate between the Right and the Left, instead advocating their own programmatic interests, was a major factor in the paralysis of the system in the 1960s that led to the unprecedented military coup of 1973. He suggests that the "Christian Democrats played a different role than did their predecessors at the center"—not only that they were more ideological than the radicals but that "they were ideological in a different way, one that can be traced, ultimately, to the way they defined themselves in terms of the predominant axis of cleavage" (p. 186). The author tantalizingly suggests some universal implications that transcend the Chilean experience, an argument to which his analytic framework inescapably leads him; but these are not well explored. A brief discussion of postmilitary politics in Chile and speculation about "future patterns" is included at the end of an all-too-short final chapter; but it adds little beyond updating and is a letdown after the often cogent analysis that precedes it.

The intellectual framework the author uses to organize his analysis is appropriate if rather simplistic. Anyone familiar with Chilean political history, as Scully clearly is, would probably reach about the same conclusions he does with or without the cleavage and critical juncture concepts. However, the framework is tactfully used and may help explain the vicissitudes of Chilean political history for the uninitiated. The theoretical implications of the work, particularly the explanations for the chronic political paralysis and the collapse of Chilean democracy in 1973, as well as the role of center parties generally in other multiparty political contexts, plead for more comparative analysis than the author is able to provide; but he is aware of their importance and provides valuable assistance for those wishing to pursue them. By so doing, he makes a useful contribution to the study of comparative party politics.

Syracuse University RONALD H. MCDONALD

An Educational War on Poverty: American and British Policy-making, 1960–1980. By Harold Silver and Pamela Silver. New York: Cambridge University Press, 1991. 447p. $64.95.

This book provides a comprehensive, if not exhaustive, history of poverty-related education policy in the United State and Britain. Although the subtitle indicates a 1960–80 time frame, the work often begins earlier as it explores various aspects of the relevant wars on poverty. The authors' claim that the book is the product of 10 years of research is clearly verified by the depth of the scholarship and the superb use of primary historical materials.

The book is in the finest tradition of comparative public policy research. It also serves as a reminder, to those of us who tend to concentrate on single-nation studies, how valuable such comparative studies can be. That there are very few of these types of studies, relative to single-nation studies, is an indictment of current public policy research.

The book goes well beyond a documentary of the legislative efforts surrounding the problems of education and poverty. The book does include that documentation. However, the majority of the book describes and analyzes the role of research and experimentation and interventions by nongovernmental entities and important government commissions and committees. The most detailed example of the latter are approximately three chapters devoted to the British Plowden Committee report, *Children and Their Primary Schools* published in 1967. That massive study was commissioned by the British government and laid the groundwork for British efforts in the 1960s and 1970s in dealing with poverty-related education problems.

Readers versed in either the American or British cases may find some material rudimentary; but because of the comprehensiveness of the volume, they are still likely to stumble across new information on the wide range of research and intervention covered. For example, although most American readers will be well aware of the contribution of Michael Harrington's *Other America* or the importance of white flight in the fight against poverty, they may be less aware of the prewar research on early childhood development or the details of the Ford Foundation's interventions in a number of cities in the 1950s.

One of the intriguing aspects of the book is the interplay (or lack thereof) between American and British policy and research. They argue that initially there seemed to be very little cross-fertilization but that over time, as policy communities began to be aware of, and interact with, each other, important cross-national links emerged.

Somewhat annoying, in this extremely detailed and comprehensive history, is the authors' apparent surprise at the contextual differences between the two countries' views of poverty and educational inequality. For example, throughout the book, they emphasize the lack of a substantial federal role in primary and secondary education in America. This certainly is a worthy contrast with British, and even more with other European countries. However, it is hardly a revelation to American education experts. In addition, the effects of this on policy formulation and implementation are not as clearly explored as they might have been. It is exactly these types of contrasts which provide the most appealing possibilities in comparative public policy research.

The Silvers appear even more surprised at the differing explanatory contexts for poverty in the two countries. They repeatedly note explicitly the lack of a systematic, class-based explanation for poverty and educational deprivation in America. They quite accurately portray the language and rhetorical differences that emerge in both the research and policy debates over poverty in the two countries. But again, this should surprise no one. Americans have always viewed their culture as less class-based than other nations view either their own culture or ours. And this is particularly true of England, especially English education. Again the importance of this not-surprising revelation is not as clearly developed as it could have been.

Although reading straight through this book will not be easy for many readers and it is very easy to get lost in the massive detail, the volume is an extremely valuable resource that readers will return to on numerous occasions. Finally, any review of this book that did not mention the 90-page bibliography would be woefully incomplete. The bibliography alone is worth the price of the volume.

University of Wisconsin, Madison JOHN F. WITTE

Patronage and Politics in the USSR. By John P. Willerton. New York: Cambridge University Press, 1992. 305p. $59.95.

One strong research tradition in Soviet political studies was the literature on the relationship between power consolidation and authority building, that is, how leaders expanded their power by building extensive networks of clients and allies who shared their policy outlooks. This topic became the subject of a number of excellent works on the Khrushchev and Brezhnev leadership periods. Willerton's study of clientelism represents, however, the most systematic examination of this aspect of Soviet politics. Collecting and coding data on careers of officials who reached the top level of power in the USSR (and in two of its republics, Lithuania and Azerbaijan, included in order to compare two subcentral units with the union-wide system), Willerton identifies the major patronage networks that linked senior party leaders with their protégés and clients at lower levels. Inevitably there is a certain amount of arbitrariness about the coding rules used; but Willerton's procedures make it probable that his results err on the conservative side and that the actual extent of patronage was wider, if anything, than what he reports.

Willerton makes clear the source of Brezhnev's remarkable staying power. He estimates that by 1981, over one-third of the members of the Politburo were tied to the general secretary by relations of mutual obligation and support. He notes, as well, however, that the enfeeblement of Brezhnev and other members of the leadership in the late 1970s and early 1980s weakened the dependence of clients on their ailing patrons and provoked new strains in the ruling coalition. Gorbachev employed traditional methods to decimate the Brezhnev network and to create alliances of his own—although his, as Willerton demonstrates, were often alliances of independents. Ambitious outsiders not formerly dependent on Gorbachev joined with him to cut out the rot in the system. As Gorbachev weakened and his policies faltered, his coalition melted away. Moreover, with the structural changes Gorbachev introduced (the shift from party to state power, the new importance of elections and parliamentarism), the nature of patronage politics changed substantially as the party's *nomenklatura* system no longer controlled the leading posts in state and society.

Willerton offers a persuasive analysis of how Soviet patronage politics provided leaders with a needed policy consensus and an informal means of offsetting the rigidities and information blocks endemic to the system. He does not, however, discuss how the reliance on patron–client networks proved dysfunctional to the system and ultimately weakened it by undermining the center's power to hold lower officials accountable for performance. The very stability of the Brezhnev coalition worked perversely to undermine the stability of Soviet political system. In his resignation speech of 25 December 1991, Gorbachev wistfully noted that he could have simply "reigned" for a time (as he implied Brezhnev did), instead of trying to fix the country's problems. But how long could he have done so before the accumulated contradictions of the system brought him down?

Emory University THOMAS F. REMINGTON

INTERNATIONAL RELATIONS

Restructuring of Arms Production in Western Europe. Edited by Michael Brzoska and Peter Lock. New York: Oxford University Press, 1992. 240p. $51.00.

Among the industries of Western Europe, the armaments sector generates among the most fascinating patterns of national policymaking. Producers and procurers of military goods are currently caught in a swirl of strategic alliances, multinational negotiation, industrial restructuring, and redefinition of policy. Superimposed on a vicious circle of increasingly complex technology, rising unit costs, declining production runs, and surplus capacity that has bedeviled the industry for decades, the last five years have seen the challenge of civilian multinationalization under "Europe 1992," the collapse of Less Developed Countries demand for imported arms, the reemergence of the United States as a dominant global arms exporter, the decline of procurement budgets as publics claim their post-Cold War "peace dividend," and the redoubling of efforts to foster trans-European cooperation through multinational institutions, most notably the Independent European Programme Group. A restructuring process of continental proportions is the result.

This book is indispensible to a proper understanding of this complex process. Those familiar with Stockholm International Peace Research Institute publications will not be surprised to find it filled with detailed data on policy and commercial developments in this fast-moving sector, compiled by an impressive multinational team of leading academic and public policy experts. The nature of the exogenous shocks and their impact on the industrial structures of European countries are carefully charted. Country studies chart distinctive national responses: privatization in Britain, concentration in Germany, export promotion in France, stagnation in Italy, modernization in Spain, vacillation in Sweden, and marginalization in Greece, Portugal, and Turkey.

Those who do not follow the armaments industry may be surprised that such detailed empirical research is possible, given the sensitive nature of the goods involved. Yet the arms industry is in many ways *easier* to study than comparable civilian high-technology sectors. Weapons systems tend to be large, lumpy goods with long development times. Procurement must often be approved by parliamentary committees, where it often becomes the object of spirited public debate. Multinational projects generate acrimonious diplomacy, in which selective leaks to the press (both by governments and by firms) are recognized tactics. The result is a rich body of information, making books such as this one possible.

Like nearly all studies of the global arms industry, however, this book remains oddly apolitical. Armaments production raises fundamental questions of comparative and international political economy: How much influence do different domestic actors—heads of state, ministries of defense or industrial firms, parliaments, and voters—wield over industrial restructuring? Why do governments respond in different ways to global surplus capacity? How do increasing returns, surplus capacity with high fixed costs, and other market imperfections affect government trade policy? Do mercantilist policies reflect concerns of national security or industrial policy?

Yet this book stops well short of systematically posing—let alone answering—these theoretically (and politically) critical questions. Restructuring decisions and adjustment strategies are presented as policy dilemmas of optimization against an increasingly difficult fiscal environment, rather than as a source of political conflict between winners and losers. A coherent explanation for the persistence of massive waste that every author criticizes (estimated at one-third of European defense spending) thus eludes them. The final chapter offers summary, not synthesis.

Perhaps the most troubling consequence of this atheoretical mode of inquiry is the tendency of many authors in this book (often, one suspects, against their underlying beliefs) to cite uncritically the justifications of governments for autarky. Most, for example, assert that collaborative production is less efficient than single-country production, a proposition that has been, in my view, empirically disconfirmed. Several authors assert that French opposition to internationalization results from the desire, backed by the military, for "national independence," a claim that is flatly inconsistent with the position of French ministers of defense and prime ministers over the past two decades, as well as the French Navy's public preference for the McDonnell-Douglas F-18 over its indigenous rival, the Dassault Rafale.

All in all, then, those interested in the West European arms production will find this book a useful source of reliable data and interpretation. Political economists, however, will be frustrated by its lack of systematic political analysis. This failing is hardly unique to this book: it is common to the entire genre. Serious social-scientific analysis of European armaments production has, with very few exceptions, yet to be attempted, despite the high level of expertise enjoyed by many of the authors assembled in books such as this one. A wealth of tools for this enterprise can be found in contemporary theories of comparative foreign economic policy and international political economy, including endogenous theories of protection, strategic trade theory, interest group theory, and formal models of regulation. Armaments production—for a half-century the most important single area of industrial policy in nearly every Organization for Economic Cooperation and Development country—remains an unanswered challenge to modern social science.

Harvard University ANDREW MORAVCSIK

The Nature and Practice of Flexible Response: NATO Strategy and Theater Nuclear Forces since 1967. By Ivo H. Daalder. New York: Columbia University Press, 1991. 411p. $49.50.

Ivo Daalder's new book represents a valuable addition to the literature on NATO and strategic studies more generally. Although largely limited to the nuclear aspects of NATO's military posture since the late 1960s, *The Nature and Practice of Flexible Response* arguably constitutes the most important study of the alliance since the publication of Robert Osgood's *NATO: The Entangling Alliance* (1962). Given the paucity of declassified materials from the period, the book makes excellent use of the available primary and secondary sources. And it goes well beyond most works on NATO in terms of its analytical aspirations.

The principal contributions of the book are two. First, it brings together in one place discussions of all the major nuclear issues that roiled the alliance during the two decades following the adoption of the strategy of *flexible response* in 1967. Here can be found detailed analyses of NATO's problematic efforts to develop political guidelines for the employment of nuclear weapons, the unsuccessful United States—sponsored attempts to restructure the alliance's nuclear forces that culminated in the neutron bomb fiasco, the events leading to the two-track Long Range Theater Nuclear Forces decision in 1979, the intraalliance dimension of the subsequent Intermediate-range Nuclear Forces (INF) negotiations, and further efforts to modernize NATO's nuclear posture in the 1980s. Although some of these topics have been addressed elsewhere, Daalder's comprehensive approach illuminates the important connections between them.

Second, the book presents an innovative analytical framework that helps to impose order on a seemingly unending series of intraalliance squabbles. Daalder argues that disagreements both within and between member countries over the implementation of flexible response stemmed primarily from different assessments of the requirements of extended deterrence. He outlines four distinct extended deterrence strategies—*pure, escalatory, conventional,* and *warfighting,* all nevertheless consistent with the basic precepts of flexible response—and then systematically uses these categories to identify and contrast the positions of the participants in each of NATO's nuclear debates. At the same time, Daalder demonstrates how the imperative of maintaining alliance cohesion typically enabled the allies to achieve mutually acceptable political compromises, notwithstanding deep strategic differences.

Less successful is the book's attempt to explain why various actors held divergent strategic preferences. Daalder attributes disagreements over the requirements of deterrence primarily to underlying differences in subjective beliefs about (1) the threat, or Soviet intentions, and (2) the possibility of controlling nuclear escalation. He goes on to locate each of the four extended deterrence strategies in a different corner of the two-by-two matrix defined by assigning dichotomous values to each of the two perceptual dimensions (e.g., high and low probabilities of attack and of escalation). In his subsequent explication of the four strategies, however, Daalder makes little reference to differences in threat perception (pp. 43–63). Instead, the strategies are portrayed as differing almost exclusively in terms of the threshold beyond which escalation is assumed to become uncontrollable. Thus, by the end of the chapter, they are arrayed along a one-dimensional continuum.

The limitations of this framework become clearer in the historical chapters. For example, Daalder argues that by the late 1980s, the NATO strategy debate was essentially reduced to a dispute between the proponents of conventional and warfighting deterrence. This characterization of the debate at a time when the threat to Western Europe had reached a postwar low seems puzzling, however, since these two strategies were initially defined as assuming a relatively high probability of attack. Surely, moreover, advocates of pure deterrence could have been expected to offer opinions on what to do about short-range nuclear forces following the INF treaty, since they are described elsewhere as favoring the deployment of such weapons, in particular (p. 48).

One also wishes that the analysis of the determinants of strategic preferences had been taken one step further. The sources of perceptions about the nature of the threat and the probability of escalation are left unexplored, as is the possibility of motivated bias. At the same time, the book could have devoted much more attention to assessing the influence of more objective determinants, especially those that might be expected to differentiate national positions. After all, strategic preferences reflect not only assumptions about what deters but also one's estimate of the costs and benefits associated with different strategies. While also necessarily somewhat subjective, such estimates will be strongly shaped by such enduring factors as a country's location, its resources and technological endowments, its historical experience, and the existence of foreign policy interests other than deterrence.

Indeed, the importance of such determinants is strongly suggested by the discussion of the four extended deterrence strategies. For example, the principal difference that emerges between pure and conventional deterrence concerns not Soviet intentions (as indicated by the matrix) nor even the probability of escalation but the perceived *desirability* of escalation and thus the need to take steps to make uncontrolled escalation more or less likely. This perception is strongly influenced in turn by a country's exposure to different forms of attack. Similarly, we might expect countries to prefer strategies that shift the costs to others or that maximize their prestige and influence. Of the various objective factors that could influence a country's choice of strategy, however, Daalder mentions only geography—and then only briefly, in the theoretical section of the book.

Reference to these conceptual shortcomings should not obscure the overall conclusion that *The Nature and Practice of Flexible Response* makes a significant contribution, thanks to its thoughtful analysis, thorough research, and considerable breadth. And Daalder's attempt to put the study of NATO on a more theoretical footing, even if not fully successful, is to be applauded.

University of Virginia JOHN S. DUFFIELD

Choosing Sides: Alignment and Realignment in the Third World. By Steven David. Baltimore: Johns Hopkins University Press, 1991. 247p. $38.50.

Quasi-States: Sovereignty, International Relations, and the Third World. By Robert H. Jackson. Cambridge: Cambridge University Press, 1990. 225p. $49.50.

The end of the Cold War and the collapse of the USSR invite reassessment both of Third World politics and of the interests and policy agenda of the United States in Asia, Africa, and Latin America. Both works under review contribute substantially to this reassessment. In addition, both make important contributions to international relations theory as it concerns the Third World.

David's book examines theoretical approaches to the alignment and realignment behavior of Third World states. David rejects balance of power and balancing approaches to the issue of alignment, arguing that their systemic, state-centric approach pays insufficient attention to the attributes of Third World states. The threat to the *leader*, not to the state, is the principal determinant of alignment decisions. Internal weakness implies that domestic threats to the leader are as important as, or more important than, external ones. Balancing and bandwagoning theories ignore these internal aspects and are consequently incapable of adequately explaining or accurately predicting the behaviour of Third World states. Implicitly, he takes the view that policy is excessively influenced by these bodies of theory. To the extent that they provide deficient structures with which to understand the behavior of Third World states, they favor faulty policymaking.

The author argues that alignment decisions are the result of balancing against both internal and external threats, whose salience varies over time. Leadership decisions reflect a judgment as to what alignment is most useful in addressing the most salient threat of the moment. This often involves appeasing one threat in order to resist another. He demonstrates these propositions through an examination of three cases: (1) Sadat's Egypt, (2) Somalia and Ethiopia under Siad Barre and Mengistu, and (3) Nimeiry's Sudan.

There are a number of shortcomings in the work. The choice of cases is limited to northeast Africa, raising questions about the universalizability of the argument. The author is aware of this problem and is careful to justify his choice. None the less, given the growing diversity of the Third World, a broadening of the sample would have rendered his effort to generalize more credible.

Moreover, case study theory suggests the utility of choosing at least one "crucial" case in which the hypotheses are least likely to hold. A possibility is the realignment decision of Castro in 1959–61, which was apparently driven almost exclusively by his perception of an external threat from the United States—or for that matter, Castro's failure to attempt realignment or ideological redefinition at the end of the 1980s as Soviet support declined and then disappeared.

Second, there is an element of the "straw man" in his characterization of established theory. He exaggerates the degree to which balance of power and bandwagoning theories are inattentive to internal threats. Each pays some attention to indirect intervention through the support of insurgency or opposition as an instrumentality that states use to threaten other states and to which the target responds through alignment behaviour.

Moreover, it is questionable whether theory oriented toward external threats has, in fact, distracted the superpowers from the internal threats faced by friendly leaders. Numerous cases suggest ample American awareness of the internal dimension of threats to Third World leaders and either the willingness to exploit it (e.g., the Reagan Doctrine) or to strengthen allies against it (e.g., American counterinsurgency assistance to Mobutu Sese Seko and to Latin American regimes in the 1960s). Neither theorists or policymakers are as myopic as they are made out to be. Nor is theory as important as he suggests.

The essential differences between his approach and those he criticizes are, first, that international relations theory, when it considers internal threats, tends to see them as extensions of external ones and ignores or underestimates purely internal threats. David rightly emphasizes that many internal threats have little to do with external determinants but are nonetheless important aspects of the calculus of foreign policy. Second, balancing and bandwagoning approaches to the explanation of alignment do not ignore, but do understate, the importance of internal politics as a determinant of foreign policy. The author makes a significant contribution to the literature by challenging and correcting this flaw.

The essential problem with conventional international relations theory as it relates to the Third World lies in the projection of Eurocentric assumptions about the state (in particular, about internal sovereignty) that do not obviously apply to many Third World states. David's book displays a laudable sensitivity to the domestic weakness of Third World states and the implications of this condition for their foreign policy behavior.

This essential characteristic of Third World states brings me to the second work under review. The vulnerability of Third World polities raises two questions: Why are these states weak? and How do they survive in an anarchic international system? Jackson's book addresses the latter of these questions. Whereas David's analysis falls squarely into the realist tradition, this book is self-consciously idealist in the author's conviction that ideas have an important role in the explanation of social outcomes.

His essential argument is that weak Third World states have been allowed to emerge and have survived because of a fundamental normative change in the international system. Sovereignty in classical international relations involved both a negative aspect (freedom from intervention) and a positive one (a capacity on the part of the state to act—to exert control over its domestic affairs and to defend itself externally). He posits that norms surrounding sovereignty have evolved in such a way as to recognize the right of any colony to self-determination and equality of status in the state system independently of considerations of capacity to govern or defend itself. This basic normative change immunizes weak states from external threat.

Beyond this, the inclusion in the international system of a new class of states incapable of exercising positive sovereignty and wide recognition that prior colonialism carries with it some obligation to redress the weaknesses of postcolonial states have created in the North a recognition of the imperative of assisting new states and in the South a sense of entitlement to redress. In this sense, Third World quasi-states benefit from the full rights associated with negative sovereignty and, in addition, from the entitlements perceived to follow from their weakness. This has led to a fundamental erosion of reciprocity in international law and practice. The author discusses the implications of this evolution for the international economy and for human rights.

The author concludes with an examination of the implications of change in sovereignty norms for international relations theory. He notes in particular that power-political approaches to international theory are not of obvious relevance in accounting for the survival of quasi-states, since most of the latter have no capacity to exercise positive sovereignty to help themselves. Instead, their survival rests on "a novel kind of international idealism" (p. 171).

This book will no doubt be criticized for its evident conservatism and highly uncomplimentary perspectives on the capacities of Third World states and the propensities of Third World leaders. I do not share many of the author's views but find his candor refreshing and his emphasis that the individual is the ultimate bearer of rights to be a useful normative reminder to defenders of the sovereignty and rights of Third World states.

In essence, this work is more about law than about politics. This has its strengths and weaknesses. It is a useful corrective to the propensity (at least among realists) to ignore the significance of ideas and norms in international society. On the other hand, the author's emphasis on the power of ideas in politics may have lead him to underestimate the role of politics and political conflict in defining and maintaining the norms of sovereignty that he is discussing. Two examples will suffice.

He explicitly discounts the role of national liberation struggles in convincing the colonial powers to pull out, noting that instances where indigenous peoples mounted effective insurgency were very few. In the majority of cases, no such struggle emerged. His interpretation of the causes of decolonization rests heavily on the normative shift in the North, rather than the strivings of the South. But the paucity of cases of effective national liberation struggle is itself an inadequate measure of their causal significance. In the cases of the French, in any event, the few wars that did occur were arguably sufficiently painful to alter French perceptions of the wisdom of empire in general. In the Portuguese case, the causal role of national liberation wars is even clearer. They were bankrupting the Portuguese and causing the departure of a substantial portion of the draft-age male population from the metropole into exile.

Second, he explicitly discounts the role of superpower competition in maintaining the independence of quasi-states, again preferring to emphasize the power of norms and the force of a world opinion embracing these norms (pp. 169–70). His position appears to be that strict realism would dictate superpower indifference to many quasi-states of the Third World. That the great powers are nonetheless interested in courting Third World leaders reflects the democratic character of international society, in which opinion, as well as power, matters.

However, it is possible to account for this superpower interest in terms of the structure of bipolarity and the significance of perceived gain and loss in such systems. David's book, for example, leaves little impression of superpower indifference or of the significance of democratic norms. Superpower involvement is a product of power politics.

Moreover, it is at least plausible that superpower competition had the unintended consequence of strengthening norms concerning sovereignty and territorial integrity. It is perhaps significant that in the two most serious African challenges to the principle of the immutability of the postcolonial territorial settlement (Nigeria and Ethiopia), superpower assistance provided the central state with the means to forestall disintegration. The relationship between the politics of interest and the structure of norms in international society is reciprocal. The author does well in demonstrating the power of norms in influencing politics. He does less well with the other side of the equation.

Despite the differences between these two works, they complement each other by illuminating two interesting dimensions to the problem of weak states in the international relations of the Third World.

Queen's University S. N. MacFarlane

Superpowers and Client States in the Middle East: The Imbalance of Influence. Edited by Moshe Efrat and Jacob Bercovitch. New York: Routledge, Chapman & Hall, 1991. 272p. $49.95.

How do small, dependent powers evidence such influence over, and display considerable independence from, their superpower benefactors? *Superpowers and Client States in the Middle East,* edited by Moshe Efrat and Jacob Bercovitch, uses the concept of patron–client relations to address the "big influence of small powers" and "to differentiate between power capabilities and the exercise of influence in specific contexts . . . [and to go] a long way towards explaining the roles of the superpowers and their client states, and the capacity of each to initiate and influence events in the region" (p. 4). Unfortunately, the book does not successfully demonstrate how patron–client relations offer a discriminating tool for power analysis or that it can more fully illuminate Soviet–Syrian and U.S.–Israeli relations.

The first part of the book introduces the concept of patron–client relations. Bercovitch, in the introductory essay, defines it as a "special type of interaction between states with manifestly unequal resources linked together through joint interests or effective bonds to provide mutual services or common goals" (p. 15). Both states voluntarily and informally participate in a relationship that exchanges responsibilities and resources and exists so long as each state derives some benefits.

Bercovitch's theoretical contribution contains two principal problems. First, his approach fails to differentiate patron–client relations from other concepts in international relations that involve asymmetrical power relations—notably, alliances and dependent states. When does a power imbalance become a special case of patron–client relations? When does a client state become a client state? No method is offered for unpacking the conceptual overlap between patron–client relations and other types of dependent patterns.

This relates to the second problem. Bercovitch cannot differentiate this concept from others because he fails to locate patron–client relations explicitly within a broader environmental context; that is, to better understand the logic underlying patron–client relations in the Middle East, he needs to shift his analytical focus from the attributes and interests of the states to a greater appreciation of how their respective roles are defined by the structural context of the Cold War. Specifically, while superpowers might restrict their client's behavior in certain ways, the character of the Cold War enabled clients to act in a surprisingly independent manner in many areas. A better understanding of the behavior of clients requires greater attention to how its role (and its autonomy) was shaped by the logic of the Cold War, not just by anarchy and unequal power resources.

Philip Windsor's contribution comes closest to exploring how these roles generated by the Cold War are crucial for understanding the nature of patron–client relations in the Middle East. Specifically, he argues that patron–client relations are a special case of asymmetrical relations that emerged as a consequence of the Cold War and regional conflict: "Client states became the determinant actors in a scene which should have appeared to have banished them . . . [and] matter because they can do things that superpowers cannot" (p. 37). In short, a Cold War that paralyzed the superpowers gave birth to both subsystem dominance and a particular function for client states; these factors both provided client states with considerable autonomy and influence and help explain why "tails so frequently wag dogs"

The remainder of the book proposes to extend the concept of patron–client relations to the U.S.–Israeli and Soviet–Syrian experiences; the hope is that an examination of the relationship from each party's perspective will better identify the conditions that permit small states to wield such influence over their patrons. While providing good historical overviews of these dyads, these empirical chapters demonstrate little effort to integrate the initial theoretical chapters and, consequently, fail to produce new theoretical or historical insights.

Bernard Reich's descriptive study of U.S. foreign policy since 1967 does not discuss patron–client relations until the conclusion—and even then with only secondary interest. The chapters on Soviet–Syrian relations make little effort to consider how patron–client relations might illuminate Syria's striking autonomy from the Soviet Union. Moshe Efrat provides voluminous data on the economic and military dependence of Syria and Israel on their respective patrons but offers little attempt to discuss whether, for instance, some forms of dependence are more important than others, how the statistical flurry relates to the theoretical question at hand, or what relationship pertains between dependence and client status.

Sheffer's contribution on U.S.–Israeli relations from Israel's perspective stands alone in providing a dialogue between the concept and the historical study. He calls for greater attention to the client's perceptions, and argues that Israeli leaders have not viewed themselves as a client state with lesser power per se. On the contrary, they have continually asserted that they provide a key and critical function for U.S. containment policy. Because they have viewed themselves as equal partners, there is no reason to exhibit deference or subservience; therefore, they have not done so.

In general, while the volume demonstrates that the concept of patron–client relations holds some theoretical promise, the essays generally fall short of demonstrating the concept's full capacity to illuminate superpower–client relations in the Middle East.

University of Wisconsin MICHAEL N. BARNETT

Rivals Beyond Trade: America Versus Japan in Global Competition. By Dennis Encarnation. Ithaca: Cornell University Press, 1992. 222p. $24.95.

As so often happens these days, this book poses what looks like an economic question but comes up with an essentially political answer, one to which political scientists should pay close attention. The question is simple: Why do the Japanese sell more in America than the Americans sell in Japan? Within this primary question, moreover, there is another and more difficult one concealed; for it is not just that Japanese exports to the United States are bigger than U.S. exports to Japan but that in manufacturing, Japanese firms' subsidiaries in the United States sell more on the American market than American firms' subdiaries in Japan sell on the Japanese market and, indeed, in neighbouring Asian markets. Why is this? It is this second question to which Encarnation's painstaking study of both groups of enterprises was directed, and the bulk of the book relates the results of a large number of firm-specific inquiries in the United States and Japan.

This evidence—and Encarnation's deductions from it—are very convincing and have implications for political theory that go far beyond the apparently straightforward economic question. To appreciate that, even readers who are not interested in the detailed evidence or do not have time to read through it should look carefully at chapter 5, concerning alternative explanations, versions of which can be found in the national press almost every week: (1) that Japan sells more because the United States is a tired, declining old hegemon, past its imperial prime; (2) that U.S. firms have been handicapped in the 1980s by too strong a dollar (though that hardly holds for 1992); (3) that U.S. firms have never tried hard enough either to export to Japan or to make their products there; and (4) that the Japanese are unfair and cheat on international and bilateral agreements to open their markets. Political scientists will be familiar with each of these.

Encarnation dismisses each of the first three explanations and argues that the fourth (national character, essentially) is far too simplistic. His fifth explanation is much the most interesting because it links international relations (competitive as they now are for world market shares) with national economic policymaking, institutions, business practices, and structures. Briefly, he argues that outside their home grounds, U.S. and Japanese firms behave much the same. But inside Japan, there are many more restrictive practices, more capital controls (especially in the past), and more obstacles to mergers and acquisitions than Japanese firms encounter in the United States. The result is that you must look to firm-specific factors to explain the relatively few success stories. Some of these factors are technological. But more often, they turn out to relate to the *political* skill of the management in bargaining with the Japanese bureaucracy over terms of entry and in building coalitions with Japanese suppliers, regional authorities, and other potential allies that will oppose the power of the local oligopolists. When firms succeed in investing in offshore production in Japan, the benefits are soon apparent in trade—a point about the intimate connection between foreign direct investment patterns and trade flows that a great many conventional economists and a good many international relations scholars interested in trade have persistently overlooked. What Encarnation is saying, therefore, is that the rivalry of states cannot be understood without studying the rivalry of firms—that firms, in short, are actors in the international political economy, although the structures within which and through which they have to operate are partly set by intergovernmental bargaining (as in the United States–Japan case) or interparty, interindustry, and interfirm bargaining and also by bargaining between the agencies of governments and industrial (and other) interest groups.

The outcome for each national economy he calls the state's *strategic environmental policy*—more significant in his view (and mine) than the much more widely discussed strategic trade policy. In Japan's case, the three critical components of this strategic environmental policy have been protectionist measures, capital controls, and the often restrictive industrial structures. The importance of the latter, in Encarnation's view, contests the preeminence given by Chalmers Johnson to the Japanese state and accords better with the political economy work on industrial policy and foreign investment of Helen Milner, David Yoffie, and DeAnne Julius. But of course, Japanese policy has not been static and the account of its evolution in chapter 3 brings out the importance of good timing not only in developing countries but also in developed ones. Political skill, clearly, lay in knowing precisely when, as well as how, to moderate import, foreign investment and exchange controls and switch to more open liberal strategies. In the emerging rivalry of states, as well as firms, for world market shares, this skill and the agility in performance will be crucial.

In short, there are lessons in this book for policymakers in government no less than for academics. It has reinforced my possibly deviant convictions that political economists have more to learn from economic history, including business history, than from economic models; that transnational investment is more significant for change than international trade; and that students of government (as many of us in Britain prefer to call ourselves) have a great deal to learn from the business schools.

European University Institute SUSAN STRANGE

Danish Neutrality: A Study in the Foreign Policy of a Small State. By Carsten Holbraad. New York: Oxford University Press, 1991. 190p. $48.00.

The central argument of this book is that the foreign policy of modern Denmark, a "small state" in a strategically exposed position, cannot be fully comprehended unless seen against "the background of the long tradition of Danish neutrality in international conflict" (p. v). To be sure, within NATO and also the European Community, the Danes have shown a tendency to withdraw. Their *no* to the Maastricht Treaty in their referendum of 2 June 1992 appears to support Carsten Holbraad's thesis, namely, that Danish caution regarding European integration is "essentially a late expression of the old aversion to engaging in the international politics of the European powers" (p. 155).

Holbraad's broader interpretation of the neutralist behavior (lapsing into amoral passivity or launching into moralizing pacifism) of his native country is historically rich and psychologically complex. As a French critic of Denmark's foreign policy during the Crimean War observed, its attitude then was *craintif et optimiste* at one and the same time (p. 57). Holbraad considers these reactions two sides of the same coin, "alternative, and complementary, ways of contracting out of the international politics of a given historical situation" (p. 36). He does not attempt to specify the conditions deciding how this passivity/pacifism coin will flip. He does, however, describe the general pattern. Geopolitical "determinism," or the Danes' ultrarealistic acceptance of their physical impotence, has been most pronounced at times of high or rising tension among great powers; and ideological "internationalism," or their excessively idealistic pursuit of reform in world politics, has been most prominent during periods of détente.

As in his earlier studies (*The Concert of Europe* [1970], *Superpowers and International Conflict* [1979], and *Middle Powers in International Politics* [1984], Holbraad emphasizes the effects of the structure and state of the world system. Denmark's future, he speculates, might be determined by a "sandwich model" with the top and bottom layers of the current hierarchy of powers coming together against the middle (p. 182); that is, a fearful Denmark and other small European states might be joined by the United States and even a relatively benign Russia today against a united Germany.

In *Danish Neutrality*, a single-country study, Holbraad more heavily stresses domestic politics, particularly the programs and the partnerships formed by Denmark's old and new political parties. Although public opinion in today's democratic Denmark no doubt still is influenced by that country's categorical "small state" role in the international system, it certainly is not, as the Danish anti-Maastricht Treaty vote makes clear, mechanically governed by it.

Like other treatment of current Danish foreign policy, Holbraad's historical analysis of its course from aristocratic management, to middle-class control, to popular domination covers three centuries. He distinguishes five stages of what he unmistakably judges to be a decline, a virtual abnegation of policy. From the Great Northern War, ending in 1720, until 1807, when Denmark (then including Norway) was involved in the Napoleonic struggle, the Danish kingdom was able to secure its neutral status by participating regularly in Europe's shifting alliances and power balances. This first phase he calls *aligned neutrality*. A second stage, *isolated neutrality*, lasted through World War I. It was marked by Denmark's accommodation to the rising power of its southern neighbor, Germany, and also by a narrow focus on trade. In the third period, *defenceless neutrality*, coinciding with the lifetime of the League of Nations, Denmark's leaders ceased to rely either on their own military strength or on forcible collective security. Danish theoreticians produced an innovative theory ("neoneutrality") of preventing war through active cooperation with other neutral countries, optimally involving the remote but powerful United States. A brief fourth phase, following World War II and the traumatic Nazi occupation, was an interlude of *nonaligned neutrality*, when Denmark's anxious leaders offered their "bridge-building" services to the (uninterested) new Soviet and American superpowers.

The fifth and current stage, which Holbraad less accurately terms *latent neutrality*, opened in 1949 with Denmark's signing the North Atlantic Treaty which the government in Copenhagen felt it had to accept after the failure of its attempted mediation of a regional security arrangement among the Nordic countries. Danish "Scandinavianism," which subsequently did produce the Nordic Council of 1952 and the NORDEK customs union project, has persisted and has contributed to the Danes' resistance to stationing foreign troops or storing nuclear weapons on their land. Distant Greenland has been less emphatically shielded by Danish protests.

Holbraad suggests that the ambiguity of Danish policy—accepting Atlantic Alliance decisions but with "footnotes" (i.e., qualifications)—expresses a latent, or underlying, Danish neutrality tendency; but this is not conclusively demonstrated. He provides little actual evidence of current interest among the Danish people in neutrality as such (i.e., legal theory or formal policy).

Given all that Holbraad shows previous Danish statesmen and scholars to have done to articulate that doctrine, one might expect him to refer in his analysis of Danish foreign policy discussion to earlier Danish advocacy of neutrality, like Georg Cohn's the author of the original and still relevant *Neo-Neutralitet* (1937). Perhaps such proofs of the "late expression" of "old aversion" (i.e., present influence of past prejudice) in Danish statecraft exist; but they are not given. Holbraad's book does establish, however, the relative greatness of Danish statesmanship tradition gone by—in the phrase of Horatio in *Hamlet* (1.1.48), "the majesty of buried Denmark."

Fletcher School of Law and Diplomacy ALAN K. HENRIKSON

The Insecurity Dilemma: National Security of Third World States. Edited by Brian L. Job. Boulder: Lynne Rienner, 1992. 255p. $35.00.

This volume is an important addition to the growing body of work on security and international politics in the developing world. The intellectual challenge is set forth in K. J. Holsti's opening review of the literature that concludes, "The conceptual equipment and theoretical approaches developed to study war in the European states system and its Cold War progeny are inappropriate or incomplete for an analysis of war and security problems in the Third World" (p. 51). Brian L. Job has assembled a strong set of essays that help fill this gap, including reflections by several scholars such as Mohammed Ayoob, Robert Jackson, and Barry Buzan, who stimulated this reexamination of security studies in the developing world by their work over the past decade. In addition, Job includes an excellent literature review by Holsti and a significant conceptual and empirical study of regional organizations by Amitav Acharya. Essays on militarization by Michael Barnett and Alexander Wendt, arms transfers and production by Keith Krause, middle powers and mediation by Fen Osler Hampson, and the role of superpowers in Third World security by S. Neil MacFarlane complete the volume.

These essays bring to light several areas where new concepts and analysis will enhance our understanding of national security. The first area of inquiry relates to the nature of the state in the developing world and its relationship to society. As Ayoob suggests, the process of state making is far from complete; and, as a result, security threats in less developed countries are most often internal, rather than external. Acharya aptly places this issue in the form of two fundamental questions for security analysts: Security against what? (placing focus on the importance of internal threats) and Whose security? (pointing to the distinction between state and regime in the developing world).

A second theme that emerges from this collection is the importance of new conceptions of sovereignty and the impact of the international system and its norms on security in the developing world. Job explores the paradox of the inviolability of the state in a formal sense contrasted with its permeability in a practical sense. Jackson's contribution builds on his work on quasi-states and the importance of international norms such as sovereignty, international recognition, and nonintervention to the survival of weak states in Africa. Jackson points out the irony of an international system that provides external security to, and support for, authoritarian governments, while these governments, by their repressive policies, are responsible for internal insecurity.

A third consideration developed in this collection is the importance of the regional level of analysis. As Buzan pointed out in his earlier work, as well as in his contribution to this volume, regional "security complexes" create the context in which most states and peoples struggle for safety and survival. Buzan traces the historical evolution of regional security structures and suggests that the removal of the Cold War overlay will make regional dynamics more important in the future.

Acharya considers the implications of vulnerable regimes in weak states on development of regional institutions. He suggests that leaders in the developing world often find regional security arrangements useful for coping with domestic challenges and countering threats to regime survival. He provides a detailed analysis of the Association of Southeast Asian Nations and the Gulf Cooperation Council to strengthen his conceptual argument with empirical evidence.

MacFarlane's analysis of superpower behavior is less dated than most other writings done prior to the August 1991 coup attempt in the Soviet Union. He recognizes that the historical cycle of U.S.–Soviet competition in the developing world had been broken and presents some initial ideas on what the impact of that change will be.

This volume should be read by scholars and students interested in security questions and the international politics of the developing world generally. The papers by Holsti, Jackson, Acharya, and Buzan add new ideas to our understanding of these issues. Ayoob and MacFarlane develop some of the implications of their earlier work, but scholars will find fuller treatment of their thinking elsewhere. The chapters by Krause, Barnett and Wendt, and Hampson are narrower in focus but supplement the others by filling in several gaps.

Several themes remain underanalyzed and represent an agenda for additional research. The treatment of external roles in conflict resolution is limited in this volume to Hampson's focus on middle powers. More needs to be written on the conduct and potential of international, regional, and nongovernmental organizations that are playing increased roles on security matters as the great powers disengage from the developing world. Finally, it is time to move beyond the label "Third World" when analyzing the vast diversity of the developing world. As Buzan and others in this volume point out, the security concerns of leaders in Benin, Guatemala, Iran, Mozambique, India, Singapore, and Jamaica, for example, are extremely diverse and distinctive.

Brookings Institution TERRENCE LYONS

Security with Nuclear Weapons? Different Perspectives on National Security. Edited by Regina Cowen Karp. London: Oxford University Press, 1991. 396p. $79.00.

This work is the first of a two-volume research project entitled Security Without Nuclear Weapons. The contrast with the actual title of the volume signals, from the outset, the fundamental flaw of this edited collection—the absence of a common and consistently applied framework of analysis to answer the question why states have or have not gone nuclear. The strength of the volume hinges then on the individual chapters, which, on the whole, are of high quality. The overall product remains, however, less than the sum of its individual parts.

The volume is divided into three sections, corresponding to whether a state has gone nuclear (the United States, the now-defunct Soviet Union, Britain, France, and China), remains at the threshold (Argentina and Brazil, India and Pakistan, and Israel), or prefers nonnuclear status (Canada and Sweden). Each of these state groupings represents one chapter (two each for the United States and the Soviet Union). Contributors are left to their own conceptual devices to develop their own interpretation of the causal factors that determined a state's decisions on nuclear policy. Some identify systemic factors (Britain), others focus on perception by elites of external threats (the United States) or the military–industrial complex (Soviet Union). Status (France) or irredentism (China) are also seen to play a role.

While each of these cases is certainly different and merits special attention, it is still puzzling why the Stockholm International Peace Research Institute sponsors of the project make no attempt to build on these chapters to develop a shared understanding—or at least a clear delineation of rival schools of thought—about the causal factors that have to be manipulated to develop a global strategy capable of inducing nuclear states to decrease their forces, slow modernization, and rely less on nuclear threats in pursuit of their strategic and foreign policy objectives while persuading threshold and nonnuclear states to resist going nuclear. In the absence of such an effort, it is difficult to see how the expectations of volume 2 can be realized, viz., to develop strategies that would assure states to seek security by means other than nuclear weapons. Two chapters by George Quester and Harold Müller defining threshold and nonnuclear status, respectively, only partially fill this vacuum.

The book also has a dated quality, although composed well after abundant signs had already accumulated that the Cold War was over and that the Soviet Union was in secular decline and at the brink of imminent collapse as a state and imperial power. The volume missed the opportunity to explain the demise of the Soviet state by other factors than external threats and to stress the influence of domestic factors (e.g., internal welfare demands, pressures for political and economic reform, and democratization) on the nuclear policies of states, especially the Soviet Union. Curiously absent, too, is any discussion of such significant threshold states as Iraq, Iran, and North Korea, whose nuclear programs are clearly more conspicuous threats to international stability than those of Canada or Sweden.

One could also have expected an analysis of the potential weakening of constraints on German or Japanese nonnuclear status precisely because of the fundamental shift from a bipolar to a multipolar system, the unraveling of NATO, the potential diminishing attractiveness of the American–Japanese alliance in the post–Cold War era, and the perceptible and spreading renationalization of state foreign policies. Excellent chapters by Stuart Croft and Phil Williams (Britain) and Gerald Segal (China), and a serviceable one by Klaus Schubert (France) show that these middle powers give no indication that they will appreciably cut their nuclear forces or relax their modernization programs in the near future. They suggest that new long-term, systemic pressures may be in train to prompt Berlin and Tokyo to rethink their nuclear policies. One can hope that volume 2 will correct for these gaps in analysis and gapping holes in coverage.

The chapters by William Kincade and Robert Art on the United States and that by Sergey Koulik on the Soviet Union provide informed *tours d'horizon* of the evolution of nuclear policies of these states. Koulik's furnishes arresting details about the Soviet nuclear program during and immediately after World War II that support the thesis that Moscow was well on the way to an indigenously produced bomb without the need of foreign spies. If quarrel must be found with Kincade and Art, it turns more on their interpretation of the causes of the U.S. nuclear policies than on any failure on their part to cover the required terrain. Neither fully reconciles the seeming paradox underlying their analyses of chronic elite dissensus in the United States over nuclear policies at an announced level of policy through much of the postwar period (which both perceptively describe) and the remarkable continuity of U.S. operational nuclear policies, crystallized in the high priority assigned to military targets in the Single Integrated Operational Plan, in sustained and successful efforts to upgrade and modernize U.S. nuclear forces, in Washington's persistent attachment to first-use doctrines and to damage-limiting, escalation control strategies (signified by a MAD+++ posture) and in the relentless search for a superior warfighting and bargaining stance vis-à-vis the Kremlin. Art's reply is to dismiss the policy relevance of operational continuity in a note.

Both authors also let the Kennedy–McNamara régime off the hook as major contributors to the massive build up of U.S. offensive nuclear forces and to the articulation of MAD+++. There is a distinction without much difference between the early Reagan–Weinberger and Kennedy–McNamara rationales for nuclear weapons. Slighted too, in the analysis is the crucial influence of systemic aims, shared by Democratic and Republican administrations (save perhaps for the Nixon years). Both chapters might have been strengthened by incorporating the systemic perspective sensibly developed in the chapter on Britain.

Kincade and Art imply that the United States was a status quo power. A strong case can be made that the evolution of U.S. nuclear strategy and record of U.S. interventions around the globe suggest that the United States acted more as a revolutionary, than as a satiated, power. If so, America's MAD+++ strategy (a war-winning counterforce strategy was never possible) can be viewed not only as the result of elite compromises on operational nuclear policies and deterrence within the ruling coalition of the U.S. security community (where new weapons as often as not drove strategy, rather than the reverse) but also as a response to shared global aims of domestic elites determined to win the Cold War and to create an international system to reflect their values and interests and, in the bargain, to win elections and gain influence and ascendance within American decision-making circles.

University of Illinois,
Urbana–Champaign

EDWARD A. KOLODZIEJ

Grand Strategies in War and Peace. Edited by Paul Kennedy. New Haven: Yale University Press, 1991. 228p. $25.00.

The present volume can be seen as a form of follow-on study to Kennedy's *The Rise and Fall of the Great Powers,* seeking to present, elaborate, and apply the concept of *grand strategy* to a set of case studies dealing with great powers. Kennedy attempts to define and conceptualize grand strategy in the opening chapter and applies it to the American experience in the tenth, and last, chapter. Three studies of British grand strategy make up chapters 2–4, followed by five chapters on the "Continental powers." The latter group varies substantially in time frame and clarity of focus, with chapters on the Roman Empire, the decline of imperial Spain, the "military heritage of modern Germany" across a broad time span, France in 1914 and 1940, and the Soviet Union.

Kennedy succinctly notes the purpose of the volume—"to present the reader with historical case studies of 'grand strategy'; that is to say, with assessments of the success or failure with which various powers of Europe sought to integrate their overall political, economic, and military aims and thus to preserve their long-term interests" (p. ix). Unfortunately, this brief statement of grand strategy (elaborated in chap. 1) also provides an idea of the overly ambitious task assigned to the volume's central concept. I wish to assess the value to political scientists interested in the study of international relations, and especially international conflict, of this volume by historians (with two exceptions).

The core of such an assessment must be the meaning and utility of the concept of grand strategy. Following Edward Mead Earle and Liddell Hart, Kennedy presents grand strategy as a way to relate means to ends: "The crux of grand strategy lies therefore in *policy,* that is, in the capacity of the nation's leaders to bring together all of the elements, both military and non-military, for the preservation and enhancement of the nation's long-term (that is, in wartime *and* peacetime) best interests" (p. 5; emphasis original). "All of the elements" include the full use and assessment of diplomacy; questions of national will, morale, and political culture; and the full range of economic resources, including industry, finance, manpower, and wealth.

It is obvious that in attempting to deal with this concept, Kennedy is continuing themes and issues raised in *The Rise and Fall of the Great Powers*. The concept returns to the linkages between economic resources, military capability, and the costs of defense. It returns to questions of diplomacy, resources, and *priorities*—that is, the constituent elements of "overstretch," a central component in Kennedy's explanation of the process of great power ascendance and decline.

At the same time, it is also obvious that grand strategy is related to significant areas of interest to students of international relations: the meaning and measurement of power, the role of alliances, the relationship of power to military and diplomatic success, the relationship between domestic and foreign politics, and the relevance of such broad perspectives as realism, rational choice, and bureaucratic politics.

What appear to be strengths of grand strategy, however, are its major weaknesses. As a concept, what does grand strategy add to well-established concepts, models, or frameworks in international relations? As noted, the concept is too broad, tries to do too much, and lacks the theoretical guidance of other approaches. Broad frameworks that integrate across various levels of analysis exist, for example, in Rosenau's pretheory and in the geopolitical work of the Sprouts linking environment and environed unit. (These approaches apply particularly to chapters 1, 7, and 8). The considerations of ends–policies to means–resources is the heart of expected utility and other rational choice approaches (from formal game theoretic work, to Allison's Model I, to standard realist formulations). Problems of resource extraction—control over resources—and its consequences for control over actors and outcomes are central to the literature on power and influence (and well developed in the work of Organski and Kugler). Problems with organizational parochialism and operating procedures (as seen in the studies on the Roman Empire, Germany, and the Soviet Union) are well handled by Allison's Organizational Process Model II, especially as contrasted to the Rational Actor Model I.

In sum, while grand strategy appears to be about how military policy relates to overall governmental domestic and foreign policy, it does not specify any of the ways in which these things should be related. It lacks the theoretical specifications that other conceptual models, frameworks, and formulations provide. Indeed, none of the studies attempts to define or conceptualize grand strategy. The studies do, however, attempt to approach their subjects from some broad understanding of grand strategy. Herein lies the volume's utility; it presents a set of studies which have moved from what Lijphart has called atheoretical case studies to interpretative case studies and that now have the potential to be used by the informed reader in the role of either hypothesis-generating case studies or what Eckstein has called heuristic case studies.

University of South Carolina HARVEY STARR

A Preponderance of Power: National Security, the Truman Administration, and the Cold War. By Melvyn P. Leffler. Stanford: Stanford University Press, 1992. 689p. $29.95.

A Preponderance of Power will be of immense value to scholars interested in the grand strategy of the Truman administration. Leffler has combined a solid grasp of secondary material with a comprehensive and very carefully documented analysis of primary sources, including a vast array of previously classified documents. The result is not only a more complete record of U.S. policymakers' thinking about national security but also a more nuanced and sophisticated reconstruction of their concerns and objectives.

Leffler convincingly argues that policymakers believed that U.S. national security required "an external environment compatible with their domestic vision of a good society" (p. 13). The lessons drawn from the rise of fascism, the Depression, and World War II were that closed economic and political systems were not only incompatible with "the nation's core values" and the viability of the U.S. political economy but also created the potential for military threats that could grow into global wars of attrition *if* countries with closed systems gained control of areas with significant human and material resources. U.S. policymakers believed the Soviet Union created exactly that kind of threat because it combined "a totalitarian regime, a rival ideology, and expansive security interests" with "overwhelming power on the Eurasian land mass" adjacent to the most important industrial and economic centers outside of North America (pp. 3, 515–16). Since Soviet policymakers were believed to have concluded that they would lose a global war with the United States if they tried to take Japan or Western Europe by force, the immediate threat was not military but economic and political. The suffering created by World War II and its aftermath might lead people in Europe and Northeast Asia to reject the Western model, accept communism, and turn to the Soviet Union for leadership. Germany and Japan were regarded as the key states in rebuilding these two regions and preventing the spread of communism in Eurasia. The military and political implications of the economic rehabilitation of Germany and Japan frightened not only the Soviets but also countries in Europe and Asia that were central to the success of the Western system. The fears of those that the United States wanted to keep in the Western system were met by alliances and other security arrangements designed to demonstrate that the Federal Republic of Germany and Japan were firmly in the Western camp. These efforts inevitably worsened the perceived threat to the Soviet Union. In short, the policy initiatives necessary to rehabilitate Germany and Japan, reassure the other key states in Europe and Asia, and use those regions as the cores of a global system compatible with Western values simply did not "allow for a policy of reassurance" toward the Soviet Union (p. 516). Subsequently, the creation of the People's Republic of China and the start of the Korean war dramatically increased the stress on the economic and strategic importance of the Third World (the "periphery"). Indeed, he argues it was largely concerns about the periphery that generated the massive increases in conventional and strategic weapons in the last years of the Truman administration.

Leffler's reconstruction of U.S. policymakers' thinking raises substantial questions about the adequacy of some of our most widely used texts and the research designs used to analyze the U.S.–Soviet competition. Existing scholarship frequently focuses on the military aspects of the Cold War and stresses U.S.–Soviet dipomacy. Leffler's work clarifies the crucial role of global economic objectives in U.S. grand strategy and the catalytic effects on individual policy initiatives of exchange crises, balance-of-payments problems, and fears that Europe and Japan might lose access to Third World resources. Furthermore, because one of the most important goals of U.S. grand strategy was "the recreation of power centers in Western Europe and Northeast Asia" and because it was believed that those areas needed access to the Third World economically to generate political stability through economic growth, U.S. policymakers were often "more concerned with social, political, and economic developments outside of the Soviet Union than with events in Russia" (p. 261). Treatments of the Cold War that pay scant attention to these linkages are likely to be seriously flawed.

My principal reservation is that the key theoretical concepts Leffler uses are not up to the subtlety of the story he tells. While the title and the emphasis on *preponderance* are drawn from Nitze and others who advised Acheson, the meaning of preponderance shifts in significant ways. We are told that U.S. policy "initially . . . meant preponderance only in a defensive sense: [preventing] . . . the Kremlin [from] gain[ing] control over the preponderant resources of Eurasia" (pp. 15–16). That policy later shifted to an effort to "seek . . . preponderant power," but "preponderance did not mean domination" (p. 19). Indeed, preponderance often appears more as the consequence of the effort to support key states around the world than as the purpose of grand strategy. Furthermore, he concludes that in dealing with the allies, U.S. policymakers were careful not to "dictate" (p. 500). This sense of accomodation within the West is so strong that Leffler talks as frequently about *Western* preponderance as *U.S.* preponderance (e.g., p. 364). Elsewhere, however, the emphasis shifts back to the military dimension and the ability of the United States to "dominate the escalatory cycle" (p. 443). For preponderance to be a valuable concept, the issue areas, actors, and power relationships involved need to be handled more precisely and systematically.

Similar ambiguities exist in other central concepts. The idea of risk taking is crucial (pp. ix–x); but while risk taking is sometimes linked to the probability of policy failure, it is often used as a surrogate for highly competitive policies. This ambiguity is particularly troublesome because one of Leffler's most important conclusions is that during the immediate postwar period, U.S. policymakers believed that highly competitive policies toward the Soviet Union were more likely to succeed—had a lower risk of failure—than cooperative ones—hence the judgment that while they were often wise in their understanding of what was at stake in Eurasia, "it was their prudence that seems most striking" (p. 498). But they were also sometimes quite foolish, particularly in the Third World after 1949–50. Unfortunately, risk taking is used here to mean something else, namely, policies that had a high probability of failure and for which the consequences of failure were grave.

These shifts in meaning, along with some important ambiguities in the wording used to make summary points about the nature of the Soviet threat and the coherence of U.S. policy, increase the chances that readers who rely on excerpts or do not read carefully will miss the subtleties in the overall interpretation. That is particularly likely to occur in the early sections, where readers' interpretations are more dependent on what they bring to the book. Fortunately, when one gets further into the book, the quality of the historical scholarship is so high that the narrative overcomes the ambiguities in the key terms. The conclusion, moreover, is direct, clear, and effective.

Colorado State University Alan C. Lamborn

Friendly Tyrants: An American Dilemma. Edited by Daniel Pipes and Adam Garfinkle. New York: St. Martin's, 1991. 542p. $39.95.

This book focuses on a persisting tension between idealist and realist approaches to statecraft—how to deal with nations whose governments are not accountable to their citizens. Until the Bolshevik Revolution in 1917, the United States followed the precedent, established by Thomas Jefferson, to separate recognition of a nation from approval of its internal politics. The perceived needs of the Cold War brought the United States into cordial and supportive relations—sometimes even alliances—with régimes that ranged from the moderately repressive (e.g., Mexico) to the highly authoritarian and genocidal (e.g., Saddam Hussein's Iraq during the 1980s and up to the day before the invasion of Kuwait on 2 August 1990). The end of the Cold War may eliminate such simplistic litmus tests as "authoritarian versus totalitarian" (or that it is enough for a nation to be anti-Soviet); but it certainly has not eliminated tyrants or the dilemma of how to deal with them.

Friendly Tyrants, the result of a three-year project funded by the Bradley Foundation at the Foreign Policy Research Institute, includes 19 case studies, mostly by authors with some governmental, as well as academic, experience. The 9 cases in section 1, organized chronologically, deal with U.S. policies toward Vietnam, Greece, Nicaragua, Iran, Argentina, Haiti, and the Philippines. The second set of 10 cases, grouped into four geographical clusters (Latin America, East Asia, Africa, and the Middle East/Southwest Asia), focus on Chile, Paraguay, Mexico, South Korea, Taiwan, Indonesia, South Africa, Zaire, Jordan, and Pakistan. Several other chapters provide a "framework" for the case studies, a "historical reckoning," reflections on the friendly tyrant dilemma, and a concluding "policy primer."

Owing to the continuing relevance of the friendly tyrant dilemma, an effort to examine how it has played out in a series of case studies is to be welcomed. Unfortunately, the results are quite disappointing. There are, to be sure, some strong chapters. For example, the cases on Iran (by Barry Rubin) and the Philippines (Theodore Friend) are among the best; and "The Friendly Tyrant Dilemma Today" (Michael Mandelbaum) and a sober "policy primer" (Richard Haass) also stand out. On balance, however, the book is seriously flawed in five important respects.

First, the initial chapter, which was intended to lay out a "framework" for the case studies, does not do so effectively. Thus, it is hardly surprising that there is a good deal of variance in the cases themselves. For almost two decades, Alexander George and his colleagues have demonstrated how to undertake *policy-relevant* case studies in a systematic manner. Unfortunately, that accumulated experience is completely overlooked in this project. The weakest of the cases, on Nicaragua and the collapse of the Somoza regime, can perhaps be excused because the author passed away prior to completing his contribution. It is not clear how much the chapter was subsequently improved by the editors.

Second, the selection of cases represents some genuine lost opportunities. For example, the Middle East abounds in "friendly tyrants" of immense importance to the United States, notably Saudi Arabia. It would also have been fascinating to have a chapter on American policy toward Saddam Hussein's Iraq during the decade prior to this invasion of Kuwait. Although complete details of American support for, and assistance to, Iraq will not become available for some time, there was certainly enough information when this project started to permit the case to be included. Because Iraq represents the paradigmatic example of a core realist aphorism ("My enemy's enemy is my friend"), it would have required a project with strong realist predilections to undertake a "hard case."

Third, there are strong biases in some chapters that sometimes get in the way of serious and credible analysis. For example, the chapter on "historical reckoning" gets off to a strong start with an interesting diagnosis of the impact of American culture and society on its diplomacy. Unfortunately, it then degenerates into a tired and tiresome tirade against liberals in the media and Congress. Congressional "meddling" in foreign affairs is criticized in several chapters; but the most egregious recent examples of such meddling (Senator Jesse Helms's forays into foreign policy that have sometimes left the United States without an ambassador in key nations because he put appointments by Presidents Reagan and Bush on hold) somehow escapes attention.

Fourth, a stronger sense of history would have avoided an overdose of what historians call "presentism." The discussion of current congressional activism does not even mention the Versailles Treaty confrontation, the neutrality legislation of the 1930s, or the near-success of the Bricker Amendment. Similarly, the attack on politics in current congressional hearings fails even to note the Nye hearings on World War I, the hearings on the Pearl Harbor disaster, the Truman–MacArthur hearings, or Senator McCarthy's hearings on "un-American activities" in Washington.

Fifth, there is a studious avoidance of systematic data. In the initial chapter, Howard Wiarda presents a scheme for measuring the degree of tyranny; but his colleagues pay little attention to it. Public opinion is frequently attacked without addressing some basic questions: What are the policy-relevant measures of public opinion? Do the many polls tell us anything worth knowing?

Finally, there are a few nitpicks. Since the volume was published on 25 November 1991, it might have been appropriate to acknowledge that the Sandinista regime in Nicaragua peacefully relinquished office after elections about a year-and-a-half earlier. Also, more care might have produced a useful index. For example, as important a leader as F. W. deKlerk, frequently mentioned in the South Africa case, does not appear therein.

Perhaps the most unfortunate result of this book is that it will provide ample ammunition for critics of case studies. Such studies can play a significant role in enhancing our understanding of foreign policy and in improving the quality of policy; but this book erodes, rather than sustains, the case for case studies.

It is not quite clear to whom this volume will appeal. The cases studies do not provide enough new information or analytical insight to engage the interest of informed readers, either academic or governmental. The cases might be used for teaching; but better studies of many of the same episodes are available from the John F. Kennedy School at Harvard and the Pew Case Study Program at Georgetown.

Duke University OLE R. HOLSTI

Intermediaries in International Conflict. By Thomas Princen. Princeton: Princeton University Press, 1992. 263p. $29.95.

This book is a "must read" for students of international negotiation and conflict resolution. Princen has systematically examined a dimension of international negotiation that traditionally has been explored primarily by historians and the legal profession, namely, the role mediators play in managing international conflict. The author introduces an imaginative framework for understanding the role of intermediaries in international conflict and applies this framework to four case studies of intermediary intervention: Jimmy Carter's mediation between Israel and Egypt at Camp David in 1977–79; Teddy Roosevelt's mediation between Russia and Japan in 1904–5; Pope John Paul II's mediation between Argentina and Chile in 1978–84; and efforts by the Organization of African Unity (OAU), the British, and the Quakers to mediate in the Nigerian Civil War in 1967–70.

Princen's central argument is that the impact of mediation is directly related to the bargaining relationship the mediator has with the disputants. "Principal" mediators may have indirect interests in the issues under dispute and can bring resources to bear on the parties to the dispute; in other words, they have substantial bargaining power. "Neutral" mediators, in contrast, have no interests (direct or indirect) in the issues under dispute; and little bargaining power. They can, however, provide a "low-risk environment" conducive to compromise. Intermediaries can reconfigure a bargain, make proposals, and bargain with disputants through the use of pooled information.

Princen makes a convincing argument that the roles of both principal and neutral mediators are dynamic: they evolve as they participate. For example, neutral mediators rely primarily on information management to influence disputants. The more information they elicit, however, the greater their bargaining power and the more they behave like principals. Principals begin as fellow bargainers but attempt to shift to a facilitator role over time in order to obtain information necessary to promote their interests, as well as those of the disputants.

This volume represents an original contribution to the literature on international negotiation. The author combines a conceptual approach to understanding the role of mediators with the method of structured, focused comparison to produce a rich analysis that highlights the complexities associated with international mediation. Moreover, the selection of cases highlights the role of nonstate mediators (the Vatican, the OAU, and the Quakers)—actors who may assume increasingly important roles in the post–Cold War world. Finally, by including the Portsmouth case, the author adds a historical dimension to his analysis and thus frees the book from constraints associated with the post–World War II perspective adopted by much of the recent writing on international negotiation and conflict resolution.

In my view, the most important contribution of this book is that the author examines the *political* context of international mediation. The author's focus on the needs and interests of the intermediary, as well as those of the disputants, is evident throughout the book but summarized most effectively in the concluding chapter, where he introduces the concept of "two-way negotiating," that is, "negotiation across the table" and "negotiation back home" (p. 224). But an understanding of the domestic politics of negotiation is not sufficient to explain the success or failure of mediation. Princen rightly points out that scholars need to focus on the relationship between systemic *and* domestic factors, as well as the role of the intermediary, in explaining the success or failure of mediation.

Princen's focus on politics arguably leads to a "messier" analysis than some would like. In fact, this book may be criticized by some scholars because of its "on the one hand, on the other hand" tone. In my view, however, the author has successfully confronted the challenge of dealing with the politics of international mediation. Rather than produce a simplistic analysis of the problem that neglects politics, he has elected to highlight the complexities of mediation as a political process. The fact that he has not answered all of the questions he raises about the role of mediators should not be held against him.

The author's concluding chapter raises two questions that merit further attention. First, Princen suggests that domestic politics both imposes constraints on, and offers opportunities to, international mediators. He needs to elaborate this discussion, perhaps through two or three scenarios illustrating both the limiting and contributing effects of domestic politics on international mediation. Second, although this book was published in 1992, the author does not specifically address the implications of his analysis on the role of mediators in the post-Cold War world. The end of the bipolar system, changes in relationships among nations, the rise of nonstate actors, and the broadening of the international policy debate from strictly political-military concerns to economic, environmental, and public health matters raise significant questions about the role of mediation in international affairs. More attention should be devoted to the role of mediation in this new environment. Princen's book provides a good place to start.

Georgia Institute of Technology LINDA P. BRADY

Regulating Covert Action: Practices, Contexts, and Policies of Covert Coercion Abroad in International and American Law. By W. Michael Reisman and James E. Baker. New Haven: Yale University Press, 1992. 250p. $28.50.

In spite of its title, this book is as much about the legal status of coercive intervention in the affairs of other states as about secrecy per se, except in the chapter on U.S. domestic law. The book provides a very interesting argument about changes in norms regarding both proactive and retaliatory intervention. It focuses most of its attention (six of seven chapters) on international law. The discussion is unusual in that it is enriched by explicit examinations not only of the military mode but also of economic, diplomatic, and ideological means of influencing both states and nonstate actors. A mark of its intellectual integrity is that its concluding list of policy guidelines for covert activities will make neither conservatives nor liberals completely happy.

This book should not be approached with the assumption that it provides an uncontroverted view of the state of the international legal regime on covert activity, even though the first two chapters purport to introduce, for nonspecialists, important concepts about the nature and development of international law and covert activities. Reisman and Baker are proponents of a "contextual" approach to international law. Thus, they argue that

> lawfulness is . . . determined by contextual analysis: who is using a particular strategy, in what context, for what purpose, by what procedures, where and how, with what commensurance to the precipitating event, with what degree of discrimination in targeting, and with what effects as a sanction and what peripheral effects on general political legal, and economic processes." (p. 25)

The authors describe their methodology as a "type of modern natural law" (ibid.). Major actors and laudatory purposes are given special weight in determining what is lawful. The authors thus blend many of the assumptions of what David S. Scheffer has called the neorealist and behavioral schools in his contribution to Louis Henkin and his colleagues collection of more diverse viewpoints, *Right v. Might* (1991).

The contextual approach yields a very different conception of how international law is made (and thus of what it contains) from that of traditionalists such as William R. Slomanson, in his *Fundamental Perspectives on International Law* (1990). For example, to cite only two of the most consequential differences from the traditionalists, Reisman and Baker do not consider as legally binding much of what traditionalists would regard as treaty law, except when the essential states actually and regularly act in conformity with a treaty. They reject even more soundly traditionalists' view that to be considered customary international law, one of the elements of a pattern of behavior must be the presence of *opinio juris*, that is, behavior executed with a sense that it is rooted in a binding norm. A traditionalist would say that their contextual approach leaves out a very important element of the context: in a world without an enforcing authority, states and other actors sometimes act for political and strategic reasons in ways that they know very well do not conform to international law.

The emphasis on important actors' "operational code" as opposed to "myth system" (p. 24) yields a premier position in making law for the United States and, more controversially, even for the new post–Cold War Russia, since these two countries are key actors in the international system. When hierarchy and codes inferred from behavior combine with their normative assumptions, Reisman and Baker find a international *legal* regime on intervention (particularly covert intervention) that is asymmetrically more permissive of U.S. action than most traditionalists could accept.

The reader who accepts the initial premises will probably be persuaded by the arguments, since this is a tightly reasoned (though terse) book. For example, Reisman and Baker argue that since 1945 the developing countries have attempted to raise the threshold for allowable self-defense by states from the United Nations Charter's "armed attack" up to something resembling an armed attack by a regular military invasion. While low-intensity and covert intervention are regarded as illegal, not permitting a forcible response encourages low-intensity options and incursions by nonstate actors with territorial bases in other states, because it does not permit a legal military response by the wronged party. Reisman and Baker say that this attempt to change *law* has not been successful, in spite of its widespread acceptance by many of the states in the world and even application in several International Court of Justice cases, because *it has not been accepted by the essential actors in the system* (i.e., primarily, the U.S.) The conclusion follows from the premises, but the premises are controversial.

This book would be more satisfying if it were more extensive. It contains only 143 pages of text. The remaining 106 pages include a list of allegations of covert activity referred to the United Nations Security Council from 1969 to 1988, copious endnotes, an annotated bibliography, and an index. Each of these is useful in its own right; but much more connective and explanatory fiber in the text *explicating* more carefully how the general conclusions at the end of chapters are related to the theory and the case material would make this book both more readable and ultimately more persuasive. The shortness of the text also means that important substantive matters have inevitably been skimmed, others omitted, while the treatment of the cases on which the conclusions depend is brief in the extreme. This is a special problem in a book using *contextual* analysis. Overall, however, in spite of its limitations, I would recommend that anyone with a serious interest in intervention, especially covert intervention, take a look at this provocative volume.

George Mason University FRANCES V. HARBOUR

Power and Leadership in International Bargaining: The Path to the Camp David Accords. By Shibley Telhami. New York: Columbia University Press, 1990. 280p. $40.00.

When Anwar Sadat announced that he would visit Jerusalem to seek peace with Israel in 1977, the move surprised virtually everyone. Shibley Telhami's book asks why Egypt concluded a separate peace with Israel at the expense of its relations with other Arab states and seeks to explain the specific terms of the Camp David accords. Among other things, Telhami analyzes the foreign policy priorities of Egypt and Israel and summarizes the perceptions and goals the United States, Israel, and Egypt brought to Camp David. He argues that more than anything else, the distribution of military and economic power among states explains why Israel and Egypt concluded a peace agreement.

According to Telhami, Egypt has striven to lead the other Arab states, and Israel has sought to separate Egypt from the Arab coalition. Israel has also tried to protect its alliance with the United States and to prevent Egypt from drawing too close to Washington. In this setting, the arrival of strategic parity between the superpowers and the subsequent relaxation of Cold War tensions gave Egypt more freedom to align with either one superpower or the other. Further, Egypt's economic importance in the region declined as oil wealth accrued to other Arab states. These changes, Telhami suggests, made Egypt more willing to accept the consequences of seeking peace with Israel under the auspices of the United States.

Telhami's exposition of Egypt's and Israel's foreign policy priorities and motivations is thorough and based on close acquaintance with the region's history. His argument is scholarly and balanced, qualities to be valued in research on the Arab–Israeli conflict. For all of its strengths, a serious drawback of Telhami's argument is that he overemphasizes the role of global and regional power balances as explanatory factors and underemphasizes the role that territorial conquest and loss play in shaping states' calculations about relative power. Specifically, Telhami underemphasizes the fundamental fact that Israel possessed Egyptian land and Egypt wanted it back. In several places his argument implies that Egypt would have made peace with Israel even if Israel had not occupied the Sinai. This view is mistaken. The international factors Telhami delineates influenced perceptions and set the essential context for Egyptian–Israeli relations, but Israel's control of the Sinai is central to explaining Egyptian policy.

An explanation of Egypt's policies that disconnects its motives from its territorial concerns has wandered too far from reality and paid too high a price for theoretical elegance. Telhami is right to say that changes in regional and international power balances affected Egypt's judgments about what actions were necessary and likely to succeed, but he needs to show how these changes affected Egypt's overriding interest in the Sinai. We know that Egypt tried to take advantage of these shifts, or at least prevent them from becoming obstacles in the pursuit of its central objective. Egypt's problem was that it could not get the Sinai back on its own, so it had to decide whose help to get and what costs it was willing to pay. It had three main sources of help: the Soviet Union, other Arab countries, and the United States. The Soviet Union could help with weapons, but not in sufficient amounts. Other Arab countries could help with money, but Egypt balked at the conditions attached and did not want to become abjectly dependent on the wealthy Arab states. The United States could help with diplomacy, but seeking assistance there meant renouncing the use of force against Israel and forgoing the assistance of both the Soviet Union and other Arab states. Clearly the capabilities and interests of other countries affected Egypt's foreign policy calculations a great deal; but its moves toward peace turned on the primary goal of recovering its land, not (as Telhami suggests) on a long-range plan to restore Egypt to leadership of the Arab world. For both Egypt and Israel, judgments about relative power and about how best to pursue their own interests were more closely tied to questions of territory and to the immediate imperatives of security than Telhami allows.

When Telhami turns to the specific terms of the peace treaty, he criticizes Sadat for the mistakes he made during the negotiations leading to the Camp David accords. Begin, by contrast, used an uncompromising and legalistic bargaining style to protect Israel's freedom to deal with the Palestinians in the West Bank and Gaza. Yet Begin's gains came at a high cost, for his stubbornness alienated both Carter and Sadat; and in retrospect, his government's hardline policies toward the Palestinians seem self-defeating. Whether Sadat could have done better is problematic. Telhami is correct when he argues that Sadat was not a shrewd negotiator by conventional standards. Given his country's military inferiority, however, his achievement is remarkable. He could not make peace with Israel without alienating his Arab allies, and he could not get the Sinai back without making peace. No amount of hard bargaining would have lowered the price he had to pay for concluding the deal, namely, ostracism from the Arab League. Hence, Sadat did not lose much by virtue of his ingenuous bargaining style. His allies would not join him in the peace treaty he negotiated, and a separate peace would and did force Egypt out of the coalition of Arab states.

To sum up, Telhami maintains that the major shifts in Egypt's foreign policy, such as Sadat's peace initiative toward Israel, occurred largely as the result of international variables, not domestic and personal factors. I think Sadat's character as a leader may indeed have been critical, but the important point is that Telhami formulates his balance-of-power argument too broadly. Egypt persistently rejected the status quo after 1967 not primarily because its stature had declined or because the Soviet Union's had risen but because it had lost territory to Israel. Telhami writes that long-term changes in the international system led to the Egyptian–Israeli peace, but Egypt would not have had a good reason to seek peace if it had come through the 1967 war with its borders intact. Egypt's response to the various regional and international changes that took place during the 1970s would been different had it not had as its primary objective the repatriation of the Sinai desert.

A scholarly account of the Camp David process that is rich in detail and human interest has yet to be written. Telhami writes in the positivist, mainstream tradition of political science; and within this tradition, his book contributes to our knowledge of the environment that helped to shape the Camp David negotiations. Still, the book disappoints, because it does not tell the story of the Camp David accords in a way that holds the reader's interest. Details about personalities and events are not developed even though the author had access to many resources in the Middle East that most Western scholars could not hope to obtain. Yet Telhami does subject Egyptian–Israeli relations to a rigorous analysis. Despite the book's flaws, its theoretical argument is solidly grounded and its use of evidence impressive. For that, Telhami deserves credit.

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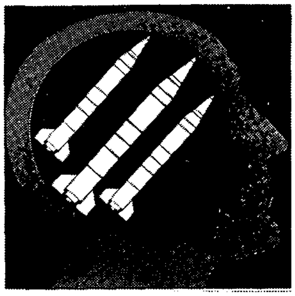

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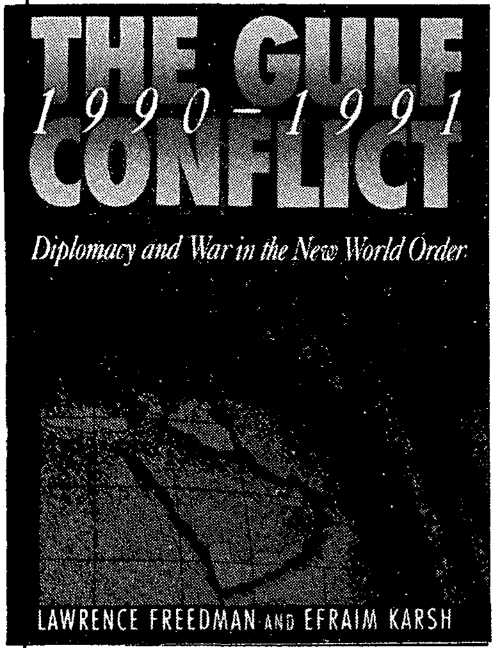
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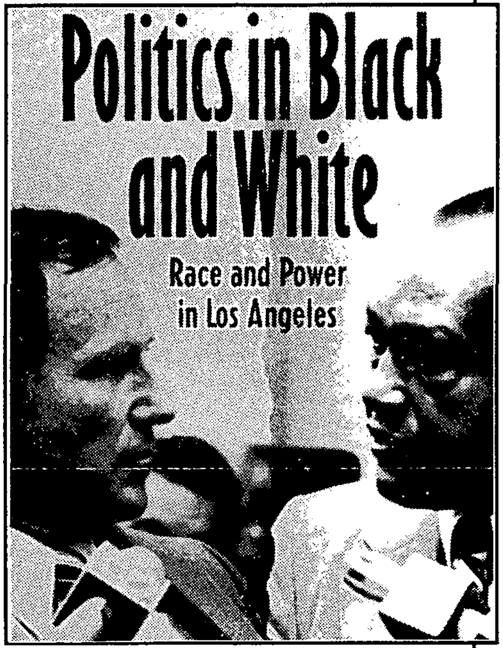

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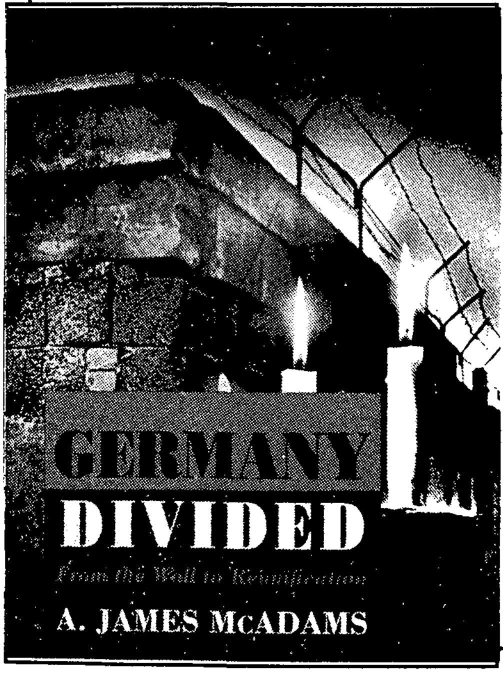

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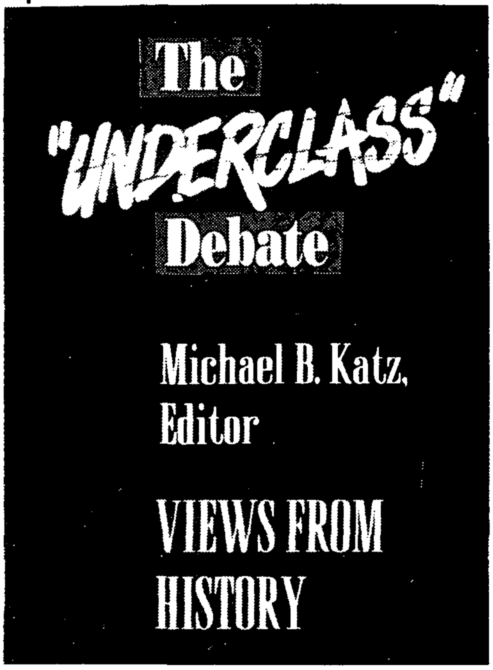
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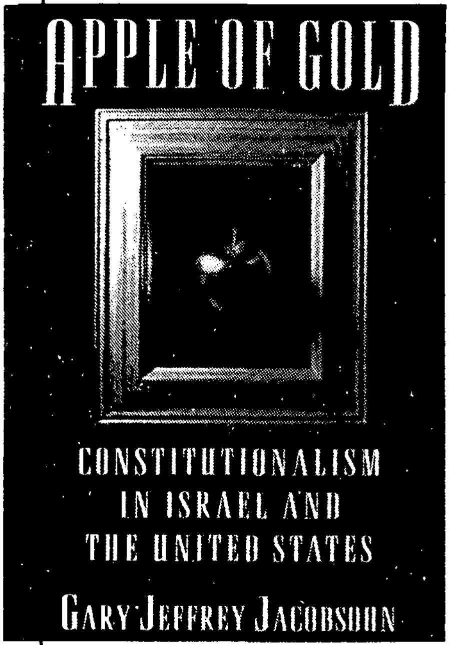

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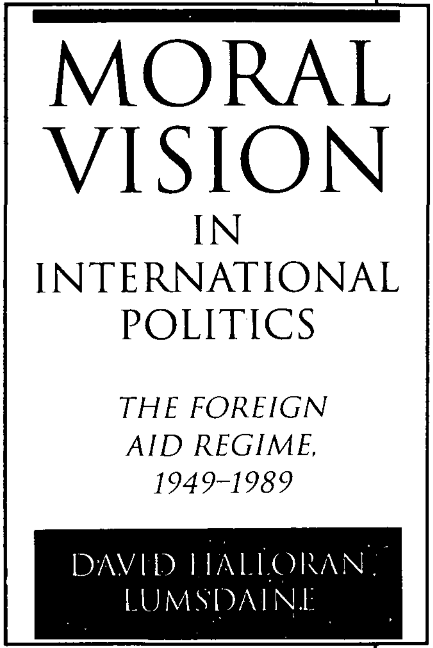
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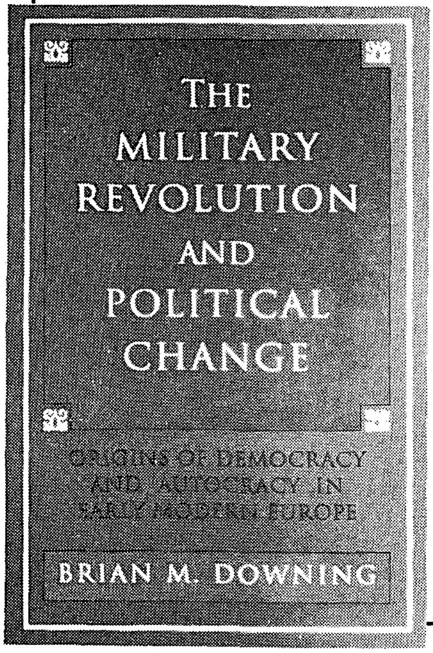
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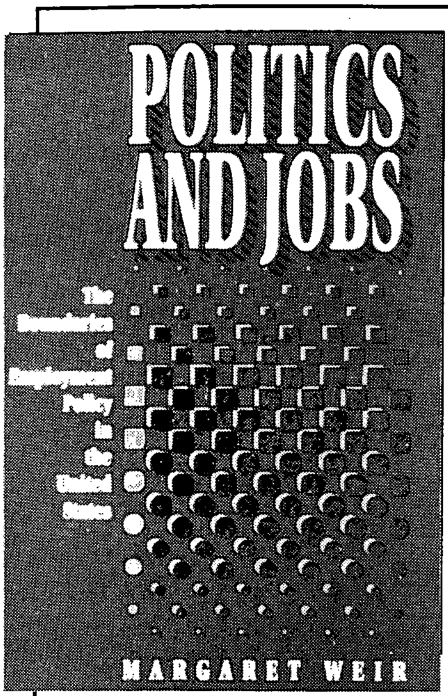
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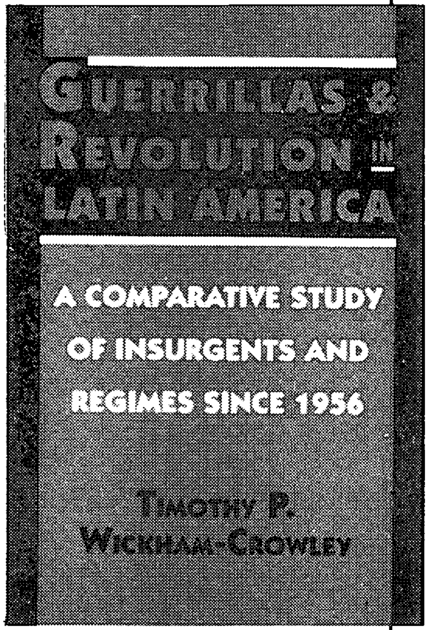
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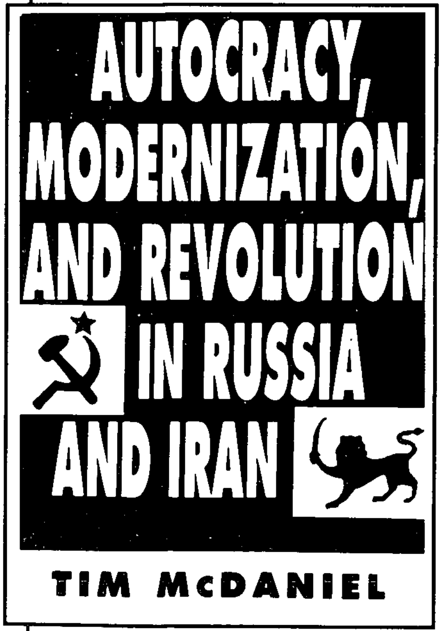
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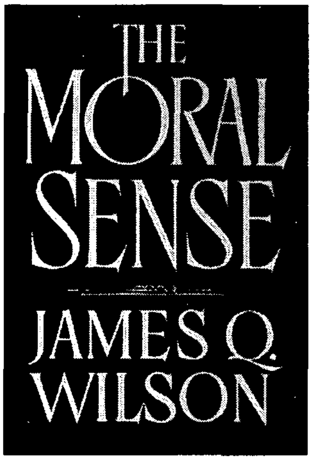

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Every day we see and hear evidence of human immorality, as images of rape, murder, and every other sort of criminal offense fill the news. Philosophers, psychologists, political scientists and other experts tell us that human nature is selfish, greedy, cruel or irredeemably neurotic. But James Q. Wilson notes a revealing paradox. He shows that while we publicly describe ourselves as though we had no moral nature, we continue to employ a moral standard in judging others and ourselves and act accordingly: most people in fact *do* behave morally. And in *The Moral Sense*—the culmination of a lifetime of reflection—Wilson sets out to restore a recognition of this capacity for human decency.

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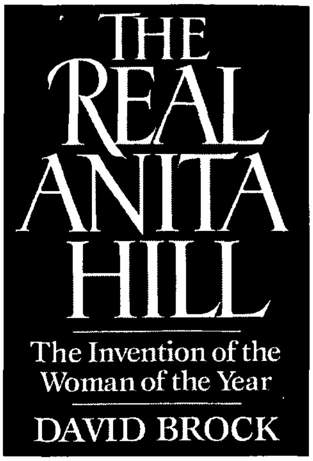

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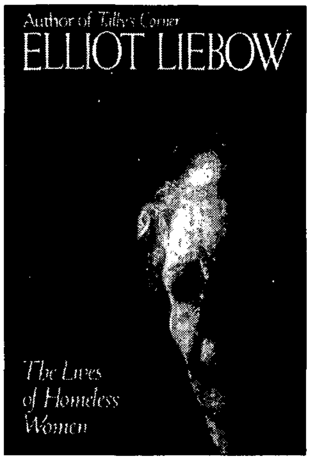

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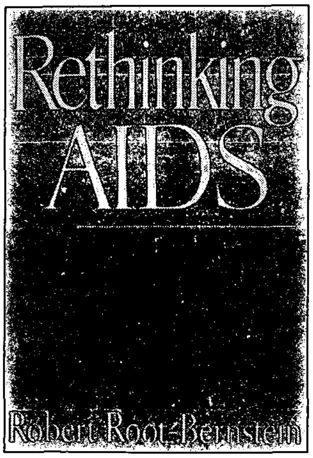

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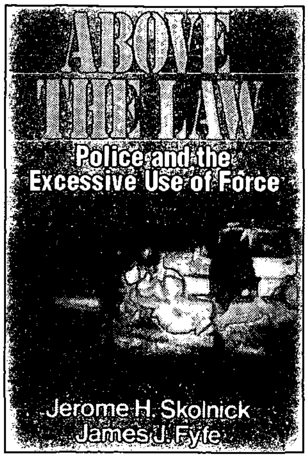

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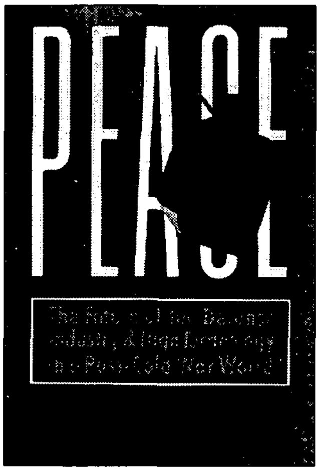

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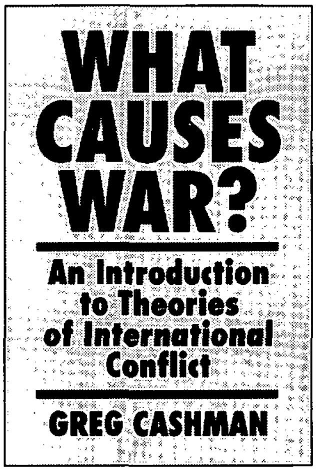

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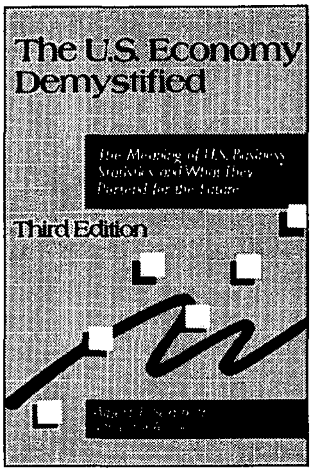

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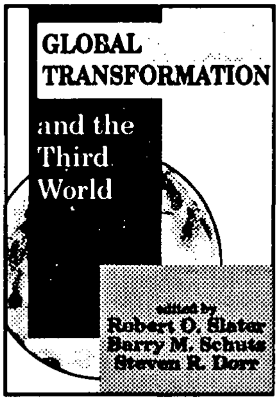
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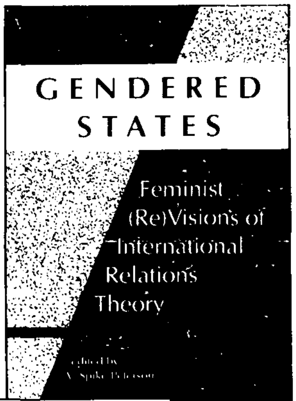
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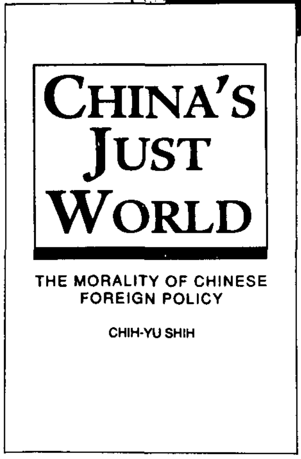
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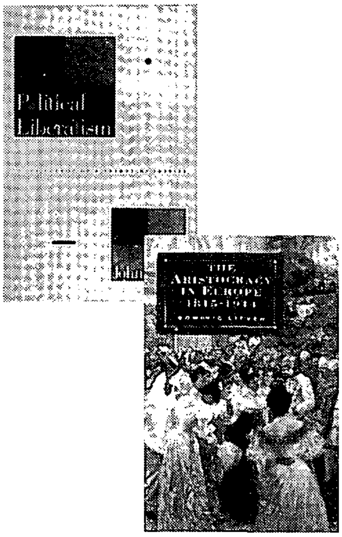
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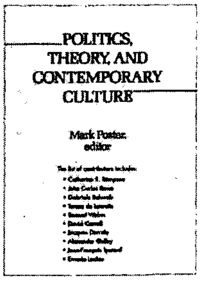
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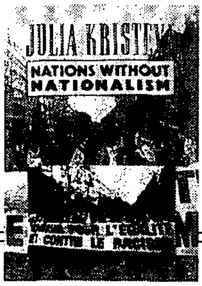
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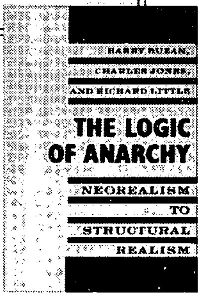
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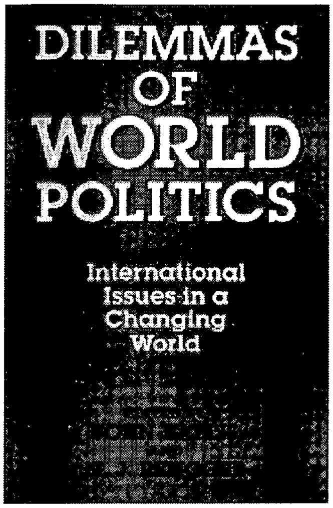
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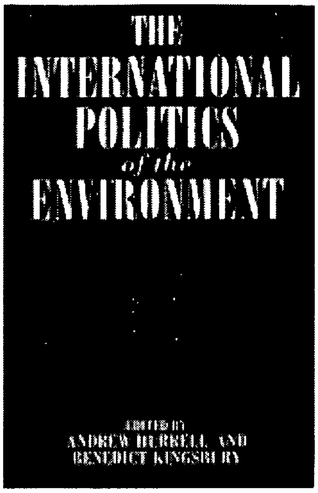
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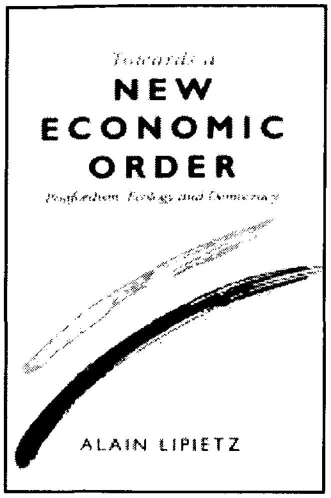

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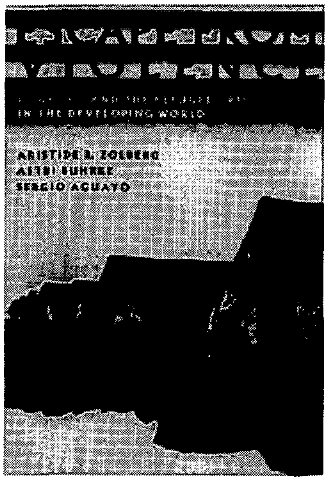

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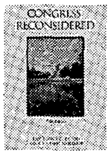

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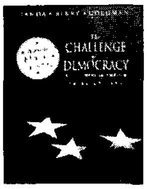

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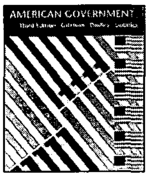

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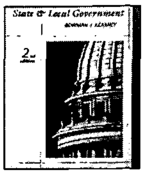

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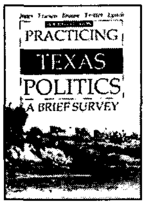

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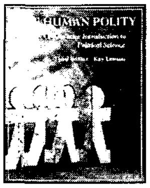

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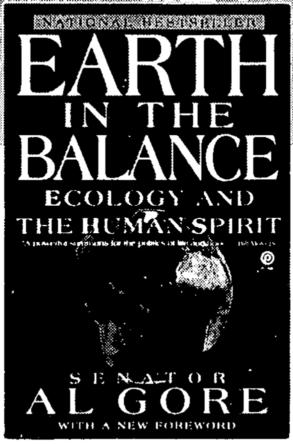
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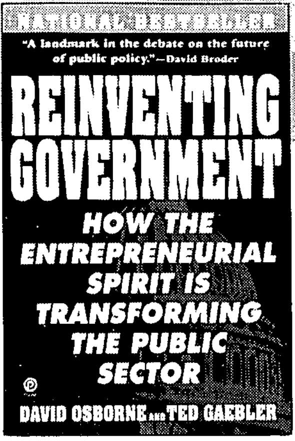
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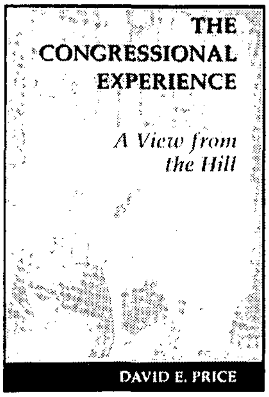

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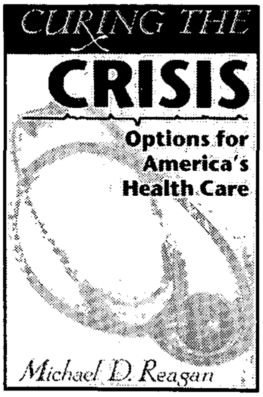

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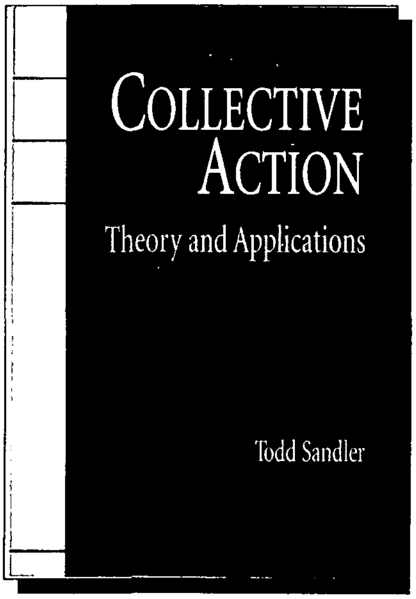

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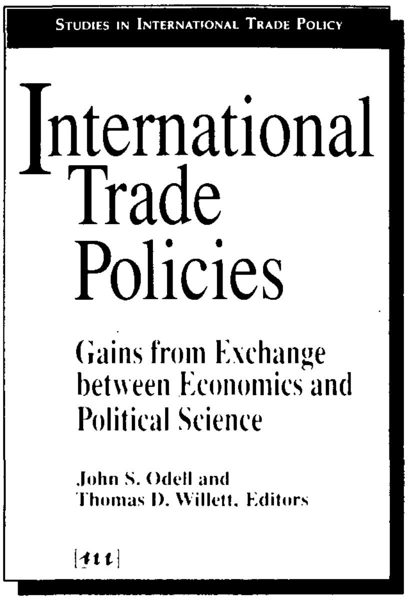
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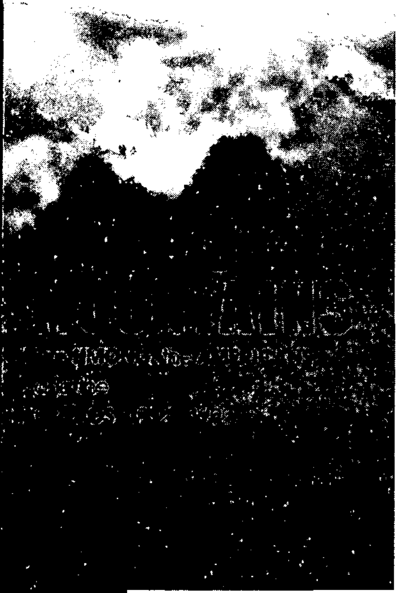

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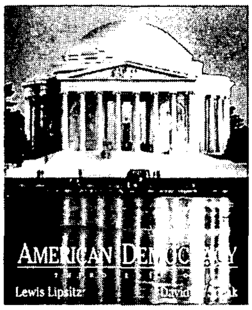

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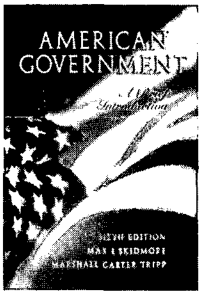

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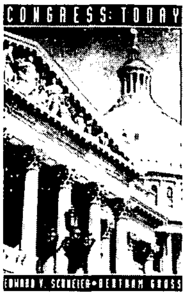

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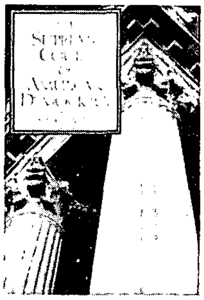

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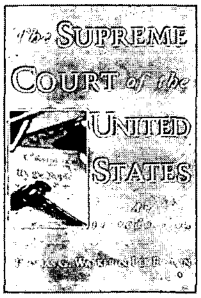

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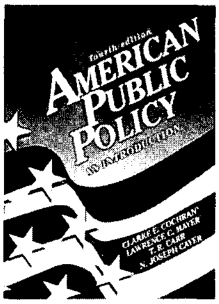

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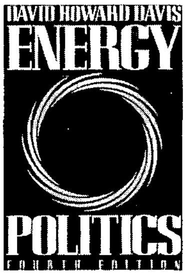

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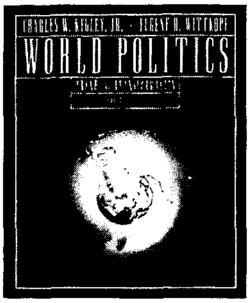

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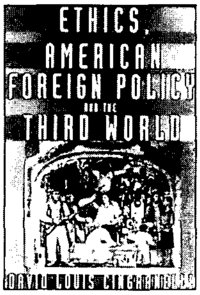

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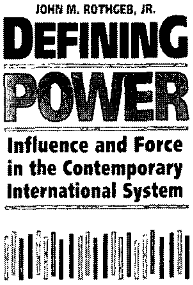
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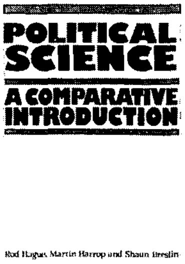
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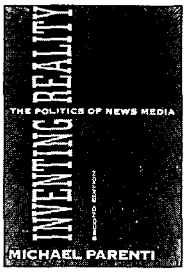
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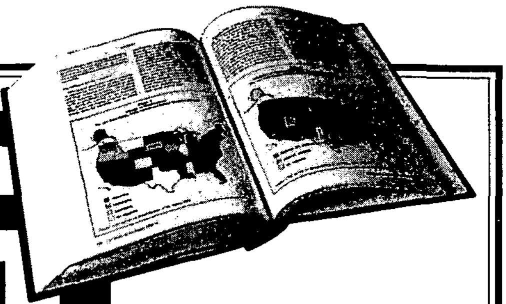

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Table of Contents

ARTICLES

The Moral Sense
James Q. Wilson 1

A Model of the Political Economy of the United States
Alberto Alesina, John Londregan, and Howard Rosenthal 12

Government Formation and Endogenous Parties
David P. Baron 34

Reconstructive Democratic Theory
John S. Dryzek and Jeffrey Berejikian 48

General Deterrence between Enduring Rivals: Testing Three Competing Models
Paul Huth and Bruce Russett 61

Is Talk Really Cheap? Prompting Conversation between Critical Theory and Rational Choice
James Johnson 74

The Supreme Court as a Countermajoritarian Institution? The Impact of Public Opinion on Supreme Court Decisions
William Mishler and Reginald S. Sheehan 87

A Theory of Voting Equilibria
Roger B. Myerson and Robert J. Weber 102

Guns, Butter, and Anarchy
Robert Powell 115

Socrates' Aspasian Oration: The Play of Philosophy and Politics in Plato's *Menexenus*
Stephen G. Salkever 133

RESEARCH NOTES

Contextual Determinants of Feminist Attitudes: National and Subnational Influences in Western Europe
Lee Ann Banaszak and Eric Plutzer 147

What Moves Policy Sentiment?
Robert H. Durr 158

CONTROVERSY

Polarity and International Stability
Manus I. Midlarsky; Ted Hopf 173

NOTES FROM THE MANAGING EDITOR 181

BOOK REVIEWS

Review Essay: The Causes and Consequences of Divided Government: Toward a New Theory of American Politics?
David W. Brady 189

Political Theory 195

American Politics 206

Comparative Politics 230

International Relations 253